THREE ESSAYS ON EMPIRICAL CORPORATE FINANCE

BY

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DISSERTATION

Submitted in partial fulfillment of the requirements
for the degree of Doctor of Philosophy in Finance
in the Graduate College of the
University of Illinois at Urbana-Champaign, 2018

Urbana, Illinois

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ABSTRACT

This thesis consists of three essays that examine empirical questions in corporate finance and labor markets.

Chapter 1:

In the first essay, I use a matched employer-employee dataset linked with hand-collected data on M&A activity in Brazil to examine how firms reorganize their labor force after takeovers. I show that M&As are associated with a significant decline in employment and total wages of target firms through increased layoffs, limited hiring and occupational consolidation. Low-skilled labor is particularly affected, while firms experience voluntary exit of high-skilled labor. Post-takeover average wages decline only for low-skilled workers. Employees that perform routine occupational tasks experience a higher likelihood of involuntary separation in transactions where the acquirer is a foreign firm. Finally, I provide evidence that occupational overlap is a key channel of increased layoffs: workers in occupations that overlap with occupations in the acquiring firm exhibit a higher likelihood of being fired. The relative increase in the demand for high-skilled and non-routine labor and the heterogeneous impact of M&As on wages leads to an increase in within-firm wage inequality. Overall, my results are consistent with a neoclassical efficiency-seeking view of M&As.

Chapter 2:

The second essay examines the link among corruption, firm growth and labor reallocation. Corrupt practices in the assignment of government contracts are largely diffused and can generate misallocation of resources across firms. I study how disclosure of such practices affects firm growth and labor reallocation. I exploit exogenous variation in the exposure of

illegally favored firms using random municipality audits by a large anti-corruption government program in Brazil. Firms exposed by the auditing program experience a decline in employment growth relative to their peers. I document that young, less-educated workers that do not occupy a managerial position have higher probability to leave the exposed firms. Released workers tend to reallocate to firms not found to be illegally favored. Within-sector firm size dispersion decreases in audited municipalities with respect to non-audited ones. My evidence suggests that random auditing programs can reduce labor misallocation across firms.

Chapter 3:

The third essay investigates the effect of founding-family control on the cost of bank debt. Specifically, I examine the cost of accessing the syndicated market by using the financial crisis and the unexpected nature of Lehman Brother's collapse as a laboratory in order to tease out the effect of family control. I find the increase in loan spreads around the Lehman crisis was at least 24 basis points lower for family firms. Furthermore, the gap in spreads among family and non-family firms becomes wider among firms that had pre-crisis relationships with lenders with higher exposure to the shock. The evidence is consistent with family control lowering the cost of accessing debt financing, especially when lenders are constrained. I further investigate potential channels that drive the effect of family control. I provide novel evidence that for 17% of the family firms creditors impose explicit restrictions in private credit agreements that require the founding family to maintain a minimum percentage of ownership or voting power. Thus, creditors value the presence of the family. Furthermore, the impact of family control on lowering the cost of bank debt is higher when family CEOs run the firms and among firms with higher ex-ante agency conflicts.

To my Family and Friends

ACKNOWLEDGEMENTS

First, I would like to extend my sincere appreciation to my Dissertation Chair, Heitor Almeida, for his continuous encouragement, support and insightful guidance. I am deeply indebted to Margarita Tsoutsoura for her unconditional support, guidance, encouragement and, most importantly, true friendship. I cannot remember how many times Margarita motivated me at critical moments during my doctoral years. Yuhai Xuan and Jiekun Huang deserve my deep gratitude for their support and guidance. I am also indebted to Jacopo Ponticelli for his valuable encouragement and guidance. I would also like to express my deep gratitude to Denise Madden, for emancipating me from logistical and administrative burdens.

Getting a Ph.D. is a long journey. I thank my Ph.D. colleagues, Tolga Caskurlu, Igor Cunha, Ruidi Huang and Ping Liu for turning this journey into an unforgettable experience with their intelligence, sense of humor and support.

Lastly, I am deeply indebted to Kate. My life would not have been the same were she not around.

Contents

Chapter 1

Corporate Takeovers and Labor Restructuring

1.1 Introduction

Corporate takeovers represent one of the most significant financial transactions among firms. In 2015 the total value of global M&A transactions amounted to $4.73T. In general, the rationale of corporate takeovers is to create value either by taking advantage of synergies or by generating efficiency gains through cost-reduction, consolidation and disinvestment. As a result, M&As affect the scope of the firms involved in the transaction and are followed by an extensive restructuring process (Maksimovic, Phillips, and Prabhala (2011)). The readjustment of firm boundaries in the post-takeover period is a subject that economists have long been interested in. Indeed, examining the reorganization process of the acquired assets in the post-takeover period contributes to our understanding of value creation sources in takeovers and has broader product market implications. Human capital is an important production factor, a scarce resource and a source of competitive advantage for firms and thus, the restructuring process is bound to entail decisions related to the labor force of the target firm, or even motivate the takeover (e.g. Dessaint, Golubov, and Volpin (2017); Ouimet and Zarutskie (2016); Tate and Yang (2016)). In addition, the labor integration and reorganization process in corporate takeovers is anecdotally a complicated and controversial issue due to the perceived adverse effects on employment. Yet, the empirical literature has largely focused on the restructuring process of physical assets of target firms, while human capital and labor-related considerations that potentially affect the reorganization outcomes have been relatively unexplored.

In this essay I empirically explore the post-takeover restructuring process of the labor force in target firms by constructing and analyzing a longitudinal micro-level dataset that combines manually-collected information on the identify of firms involved in M&A activity in Brazil and a comprehensive administrative employer-employee linked dataset. The employer-employee linked dataset consists of the universe of formal employment in Brazil and provides detailed information on individual employee characteristics and terminations of labor contracts, that allows me to capture a thorough depiction of the evolution of the labor force of target firms and characterize the extent and direction of post-takeover outcomes related to the level and composition of the labor force. Therefore, I am able to mitigate limitations in previous studies by disentangling the sources of net employment flows and exploring employee-level heterogeneous effects to document change in the composition of labor. The primary challenge in identifying the impact of M&As is selection bias reflecting the non-random nature of the distribution of M&A activity across industries and firm characteristics. However, the comprehensive nature of my dataset allows me to rely on nonlinear and nonparametric methods with the use of matching estimators and a difference-in-differences specification to address the potential selection concern, closely following the methodological approach of Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014). To empirically categorize firms into productivity types for the purposes of the matching estimator, I follow the methodology of Abowd, Kramarz, and Margolis (1999) (henceforth AKM) and instrument for productivity by using the distribution of the firm-specific wage premium. The definition of potential high-productivity firms as high-wage firms is consistent with numerous recent models of frictional labor markets (e.g., Christensen, Lentz, Mortensen, Neumann, and Werwatz (2005)), in which higher-productivity firms pay higher wages for equivalent workers. In addition, to reinforce the M&A-related interpretation of the results, I augment the empirical analysis by documenting that there are no employment effects on withdrawn M&As for reasons unrelated to labor considerations.

I begin my firm-level analysis by examining the impact of takeovers on total employment and total wages in target firms using a two-year window around the timing of the M&A transaction. Previous literature has struggled to provide convincing evidence on the impact of M&As on employment for reasons ranging from the use of only sparse employment data to non-representative samples of corporate control changes and firms (e.g. McGuckin and Nguyen (2001); Conyon, Girma, Thompson, and Wright (2002); Li (2013)). My dataset allows me to overcome this problem. I demonstrate that corporate takeovers in Brazil are associated with extensive labor restructuring at the target firm. Specifically, I find that target firms experience a large decline in employment and total wages relative to control firms in the two-year post-takeover period. The magnitude of my estimates indicate that target firms experienced on average a decline of 29% in terms of employment and 17% in terms of total wages with respect to comparable firms.

In a second step of the analysis, I transition my focus on employment flows to shed light into the sources of this adjustment. Previous literature has predominantly focused on changes in total employment. Although net employment results are informative of the direction of the post-takeover restructuring, a decline in net employment relative to the set of control firms is likely to reflect an increase in separations and/or a decline in hirings. Most importantly, an increase in separations is likely to reflect both voluntary and involuntary departures, leading to a different interpretation of the post-takeover labor-related outcomes depending on the type of separation. For example, in case M&As are motivated by cost reduction and consolidation considerations, layoffs are expected to materialize as a by-product of the restructuring process. On the other hand, M&As induce uncertainty for target employees and thus, an increase in voluntary exit of human capital is likely to lead to a decline in employment even if the motivation of the takeover is unrelated to the pursuit of efficiency gains. An important advantage of my administrative dataset is that it contains unique information on the terminations of labor contracts which allows me to disentangle involuntary from voluntary separations and, thus, document the precise manner that employment adjustment takes place. Specifically, I present evidence that acquiring firms predominantly reorganize the labor force in the target firm by significantly increasing layoffs and limiting hirings, consistent with the notion that M&As engage in efficiency-seeking consolidation. In addition, the employment decline reflects a significant increase in the rate of voluntary exit, reflecting the fact that M&As represent turbulent times for the employees of target firms. Finally, I supplement my analysis by documenting that reorganization involves occupational consolidation, as the number of occupations declines in the post-takeover period. This implies that acquiring firms not only decrease the number of workers within a specific occupation, but also that there are occupations where layoffs are accompanied by non-replacement of redundant workers, consistent with firms consolidating or limiting unrelated occupational tasks.

Having established firm-level patterns in the employment flows of target firms, I turn to an employee-level analysis to examine post-takeover changes in the demand for human capital. To the extent that acquiring firms actively reorganize human capital in the process of resetting their boundaries in the post-takeover period, I should expect M&As to be followed by heterogeneous effects on different groups of employees leading to changes in the composition of labor. For this purpose, I exploit cross-sectional variation in worker characteristics and characterize the post-takeover change in the likelihood of exit and entry for different groups of employees. To separate demand and supply side factors that affect the decision to exit the firm, I use the information on the reason for the termination of employment contracts and disentangle involuntary from voluntary exit. This cross-sectional analysis extends our understanding of the post-takeover readjustment process by providing evidence on within-firm across-employees changes in the demand for human capital.

The selection of the employee-level variation that I explore is motivated by theoretical predictions about the effects of M&As on firms. Specifically, I focus on the following human capital dimensions that are relevant to the reorganization decision of the acquiring firm: level of skill defining as high-skilled the employees that have completed at least undergraduate education, employees in managerial positions, occupational routine task intensity, and level of human capital overlap. The neoclassical merger theory is based on the view that M&As are an efficient response to regime shifts (e.g. due to technological shocks) by value-maximizing managers (e.g. Gort (1969); Mitchell and Mulherin (1996); Jovanovic, Rousseau, et al. (2001)). Therefore, takeovers potentially lead to technological change and adoption of automation, and to the extent that capital and high-skilled labor are complements, these theories would predict that M&As should generate an increase in the demand for high-skilled and non-routine labor. In addition, synergy gains have long been considered as an important driver of M&As (e.g. Andrade, Mitchell, and Stafford (2001); Devos, Kadapakkam, and Krishnamurthy (2008)). An important channel though which synergies materialize is the existence of asset complementarities between the target and the acquiring firm (e.g. Rhodes-Kropf and Robinson (2008)). Therefore, these theories would predict an increase in the likelihood of involuntary separation for workers in occupations that overlap between the acquiring and the target firm.

Given the importance of high-skilled employees for firm productivity and value creation (Abowd, Haltiwanger, Jarmin, Lane, Lengermann, McCue, McKinney, and Sandusky (2005)), I document that acquiring firms attempt to retain the target's high-skilled employees, as there is no change in the post-takeover likelihood of involuntary separation. However, given the highly liquid market for high-skilled employees and the job uncertainty associated with M&As, high-skilled employees exhibit a 15% increase in the likelihood of voluntary exit in the post-takeover period. On the contrary, given the abundance of low-skilled labor in Brazil, I find that low-skilled workers are particularly affected in the post-merger reorganization by experiencing a 35% increase in the likelihood of involuntary separation. Moreover, there is a significant decline of 18% in the likelihood of hiring low-skilled employees, while there is no impact on the likelihood of hiring high-skilled employees. These results imply that the firm-level post-merger increase in layoffs and decrease in the hiring rate, documented previously, hit low-skilled workers disproportionally, while the increase in the rate of voluntary exit corresponds to high-skilled human capital. Next, I examine the reorganization decisions of acquiring firms regarding employees in managerial positions. I find that, unlike the rest of the high-skilled employees, managers experience an increase in the post-takeover likelihood of involuntary separation, while there is no change in the likelihood of voluntary exit. This finding is in line with the Jensen and Ruback (1983) view that takeovers induce competition for the right to manage resources and achieve efficiency by replacing managers in target firms.

M&As are likely to reduce frictions associated with technology adoption and increase automation for reasons ranging from inducing a more efficient use of capital (Jovanovic, Rousseau, et al. (2001)) to alleviating financial constraints (e.g. Erel, Jang, and Weisbach (2015)). Consistent with technological change and capital upgrade, I have shown that there is an increase in the relative demand for high-skilled human capital. To further test this hypothesis, I focus on cross-border M&As. Over 90% of cross-border M&A activity in Brazil comes from developed countries, which implies that there is an increased potential for skill and technology upgrade as a response to increased exposure to trade (Verhoogen (2008); Bustos (2011)) and adoption of modern management practices (Bloom, Eifert, Mahajan, McKenzie, and Roberts (2013)). Indeed, the results indicate that post-takeover restructuring in cross-border M&As is indicative of routine-biased change with employees that perform routine tasks experiencing a 25% increase in the likelihood of involuntary separation. On the contrary, in domestic M&As there is a similar increase in the likelihood of involuntary separation for both routine and non-routine employees, implying that automation is not a driver of the post-merger reorganization process.

Finally, I exploit information on the occupational profiles of acquiring and target firms to construct a binary variable that takes the value of 1 for occupations that are present in both firms involved and 0 otherwise. I document that the likelihood of involuntary separation increases by 1.8% in the post-takeover period for employees in occupations that overlap between the acquirer and the target. Thus, the labor reorganization process is motivated by an attempt to eliminate employees in overlapping occupations consistent with consolidation and cost-reduction being one of the primary drivers of takeovers.

The employee-level results indicate that takeovers lead to an increase in the relative demand for high-skilled and non-routine labor in the post-takeover period. Turning to firm-level analysis, I examine whether these post-takeover changes in the demand for different groups of employees reflect firm-level compositional changes. I find that this is indeed the case; target firms exhibit a 7% increase in the share of high-skilled labor and a 7.6% decline in the share of routine labor. To provide additional evidence of technological change, I document that target firms increase the share of employees in occupations related to R&D by 1.2%. These compositional changes are suggestive of skill-biased and routine-biased change and thus, are expected to differentially affect the average wages of high-skilled and low-skilled employees and contribute to an increase in within-firm wage inequality. Indeed, I find that corporate takeovers are associated with a 14.6% decline in the average wage for low-skilled workers and a 4.9% increase in the average wage for high-skilled workers. Moreover, I document an increase in within-firm wage dispersion by 6.8% and the widening of the 90-10 wage gap by 13.3% in the post-takeover period, reflecting an increase in within-firm wage inequality. Finally, I demonstrate that M&As with a higher potential for labor restructuring lead to larger employment adjustments.

My essay contributes to the M&As literature that examines the post-takeover restructuring process. Focusing on physical assets, Maksimovic, Phillips, and Prabhala (2011) examine the magnitude and direction of restructuring in the 3-year post-merger period in manufacturing sectors using data from the Census LRD, and document that the restructuring process involves 46% of target plants with 26% of the plants sold and 19% closed supporting the notion that reorganization occurs in a manner that reinforces resource complementarity and exploits the comparative advantages of the acquiring firm. Kaplan and Weisbach (1992) examine long-term divestitures to document that 44% of their sample of mergers that take place between 1971 and 1982 had been wholly divested by 1989. Unlike the previous studies that focus on physical assets, my essay explores the post-takeover restructuring process of human capital.

My essay contributes to the existing finance literature that links finance with labor market (e.g. Agrawal and Matsa (2013), Simintzi, Vig, and Volpin (2014), Tate and Yang (2015), Graham, Kim, Li, and Qiu (2016), Baghai, Silva, Thell, and Vig (2016)) and specifically, the set of papers that examine the role of human capital and labor in corporate takeovers. Dessaint, Golubov, and Volpin (2017) and John, Knyazeva, and Knyazeva (2015) exploit variation induced by changes in labor regulations to provide evidence consistent with labor restructuring being a significant driver of M&As and synergy gains. Ouimet and Zarutskie (2016) demonstrate that the acquisition of valuable human capital is a significant motivation for corporate takeovers. Tate and Yang (2016) provide evidence that inter-industry mobility explains diversifying acquisitions, while Lee, Mauer, and Xu (2017) show that human capital complementarity is an important determinant of M&As. Li (2013) adds to the literature by demonstrating that capital expenditures, wages per employee, and employment in public target firms experience a decline in the 3-year period following the acquisition without a decline in the output. Ma, Ouimet, and Simintzi (2017) document the role of M&A activity as a catalyst for shifts in the occupational composition of industries and increase in wage inequality. In the private equity literature, Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014) document increased productivity and modest employment losses following a private equity buyout, while Olsson and Tåg (2017) focus on leveraged buyouts in Sweden to provide evidence of job polarization. Agrawal and Tambe (2016) document that private equity-induced investment in IT is beneficial for the career paths of employees of firms targeted by private equity.

My essay complements this literature on three distinct dimensions. First, I offer the first micro-level longitudinal analysis on the post-takeover labor restructuring process, an issue that has long concerned economists. Notice that I abstain from arguing that takeovers are exclusively motivated by labor restructuring purposes. Rather, I characterize the extent and direction of post-takeover labor-related outcomes of the average firm targeted at a takeover and highlight the role of M&As on exploiting labor-related inefficiencies. Pinning down the sources of changes in net employment has been challenging due to either the use of sparse data or the lack of detailed information on labor flows. My employer-employee dataset mitigates data limitations by tracking individuals over time and including information on the labor contracts terminations and the employment decisions of firms. Second, the disaggregate view of the labor force at the employee level allows me to document changes in the composition of labor and test predictions of merger theories. Thus, I am able to identify potential mechanisms that drive the post-takeover labor restructuring process. Third, consistent with evidence on the relationship between within-firm wage inequality and firm size (e.g. Mueller, Ouimet, and Simintzi (2017)) and in line with evidence on wage polarization (e.g. Autor and Dorn (2013)), I document within-firm evidence of increase in wage inequality after takeovers.

The closest study is the paper by Ma, Ouimet, and Simintzi (2017) that uses establishment-level data from the Occupational Employment Survey (OES) in U.S. to explore the impact of horizontal M&As on occupational employment and wages and subsequently, link the findings with within-industry occupational shifts that reflect skill- and routine-biased change. My essay complements and adds to these findings by exploiting the disaggregate view of my dataset at the firm and employee level which allows me to examine employment flows (entry and exit of the labor force) and directly document changes in the labor demand in the post-takeover period. Additionally, in my setting, I am able to track firms and employees over time at an annual basis which allows me to capture and analyze the dynamics of compositional changes in the labor force around the M&A event. Finally, I am able to precisely measure wage effects by exploiting individual-level information on wage changes and pin down the underlying channels by examining the entry and exit of employees.

1.1.1 M&A Market in Brazil

Brazil is currently the 9th largest economy in the world and is expected to experience significant growth with a forecast to be the 5th largest economy in 2050. In the early 2000s - following the successful implementation of the "Plano Real" in the mid 1990s along with privatizations of state-owned monopolies and amidst a surge in commodity prices - Brazil transitioned into a stable platform for economic growth by implementing sound fiscal policies that effected a downward trend in inflation, an accumulation of foreign reserves, a reduction in public debt, and a modernization of the credit and capital markets. As a result, Brazil was part of the high-growth BRIC countries in 2002, that, at the time, were collectively accounting for about 16% of the world's GDP growth. Since then, BRIC economies' contribution to global growth has increased to 45%. In 2006 Brazil's GDP outpaced inflation for the first time in 50 years and in 2008 Brazil became a net external creditor. In 2010 the nation was rated investment grade, for the first time, by all three main rating agencies. Thus, as a result of the fiscal, capital and credit structural reforms, Brazil has been the subject of intense M&A activity since the early 2000s.

Figure 1.1 reports the number of announced and completed M&A transactions that involve a Brazilian firm as a target, demonstrating the significant growth in the M&A market in the period from 2002 to 2014.

Except for certain regulated sectors (including telecommunications, aviation and energy) that prior authorization by the sector-specific regulatory body is required, there is no need for regulatory approvals to carry out an acquisition, unless the transaction triggers legal thresholds, and in which case the M&A transactions is generally subject to approval by the

Antitrust Authority (CADE). According to Brazil's Constitution, sectors that foreign capital is either prohibited or permitted with certain restrictions are the following:

- **Health Services:** Brazil's Constitution prohibits the direct or indirect participation of foreign companies or foreign capital in healthcare, except in cases provided for by law (Federal Constitution, Article 199, Paragraph 3). However, Federal Law 13,097, of January 19 2015, has allowed participation, directly or indirectly, of foreign capital in certain fields of healthcare.

- **Media:** Foreign ownership of open-broadcast (non-cable) media and print media outlets is limited to 30% (Federal Constitution, Article 222, First Paragraph), and 49% in cable companies with the additional restriction that the foreign owner have had a presence in Brazil for the previous ten years and the headquarters are located in Brazil.

- **Aviation:** At least 80% of the voting capital of airlines with concessions for domestic flight routes must be held by Brazilian residents, with foreign investment therefore limited to a maximum of 20% of said voting capital (Law 7,565 of November 19, 1986, Article 181, item II).

The predominant process for a foreign investor to expand activities in Brazil is the direct acquisition of an existing Brazilian entity, commonly using a preexisting Brazilian holding company as the acquisition vehicle, which receives direct investment from the foreign entity and is used as the vehicle for acquisition, and if necessary, for arranging funding. Upon acquisition, the acquirer is exposed to labor succession, as - according to the doctrine of the

Brazilian Labor Code (CLT) and the practice of labor courts - the successor company is liable for the existing labor contracts.

1.1.2 Labor Regulation in Brazil

Brazil is considered to be one of the countries with the tightest labor regulations and highest employment protection worldwide (Botero, Djankov, Porta, Lopez-de Silanes, and Shleifer (2004)). Nevertheless, firing costs are not high in practice and there are no restrictions in labor contract terminations without just cause. These are important in my essay, implying that M&As are associated with relatively low labor adjustment costs in an attempt to reorganize the human capital resources of the target firm.

The majority of the employee rights in formal employment relationships is compiled in what is known as the Brazilian Labor Code, or the CLT ("Consolidação das Leis do Trabalho"), which provides a minimum standard for employment conditions. The CLT establishes the regulations that provide for the primary labor rights granted to employees in Brazil, including legal limits of regular working hours, minimum wages, benefits, and workplace safety standards. The overwhelming majority of the labor contracts in the private sector are open-ended contracts under CLT, requiring compliance with legal termination procedures and severance compensation for employees dismissed without just cause. In addition, under CLT, employers are subject to contributions to the Social Security System (INSS), to an amount ranging from 20% to 31.8% of the payroll, and the Unemployment Savings Fund (FGTS), to an amount equivalent to 8.0% of the employee's monthly earnings deposited in a blocked account at the Federal Savings Bank ("Caixa Econômica Federal").

The employee is entitled to withdraw deposits from the FGTS account in any case of contract termination.

Generally there are no restrictions in labor contract terminations without just cause; however there is a legal process that has to be followed. Specifically, the employer has to provide a notification 30 days prior to the dismissal ("Aviso Prévio"). Nevertheless, the norm adopted in the labor market is a practice called "Aviso Indenizado", meaning that the employer is willing to pay the employee an extra 30 days of work without the employee working or being present on the premises so as to prevent problems from arising in the company by the employee being aware of his/her dismissal. Furthermore, the employer is subject to a fine of 50% of the total amount deposited into the employee's FGTS account during the period of employment. Of the 50% penalty, 40% goes to the employee and 10% to the government.

Therefore, for the median target firm in my sample that engages in 30 firings in the post-takeover period and with the median fired worker having a tenure of 11 months and an average monthly salary of R$967, the total costs of terminations without just cause in the two-year period following the M&A are equal to about R$14,500, thus reinforcing the notion of low effective firing costs observed in practice.

1.2 Methods

1.2.1 Data

This essay uses multiple data sources to create my sample. First, I use Thomson's SDC as the primary source of information on M&As. Second, I use matched employer-employee records that consist of nearly the universe of formal employment in Brazil from the Relação Anual de Informações Sociais (RAIS). Third, I utilize data on routine intensity of occupational categories from Autor and Dorn (2013).

1.2.1.1 M&A Data

Information on M&A transactions that occurred in the period from 2004 to 2012 and involve a Brazilian firm as target is obtained from Thomson's SDC. SDC includes information on both the announcement and the effective date of the transaction. I focus on both private and public targets, and consider only the transactions that were completed and involved the acquisition of a majority stake. Firms in Brazil are identified by a Tax Identifier and thus, I use the names of the acquiring and target firms to manually extract the Tax Identifier attached to the firm. The matching process involved manually scrutinizing information on the M&A transactions either provided by firms associated with the deal in the form of public announcements, or published in local government newspapers. For M&As where the approval of the Antitrust Authority (CADE) was necessary, the CADE reports were used to identify the firms associated with the transaction. I exclude M&As where the target appears to have zero employment at the time of the transaction. My sample includes 2,058 M&A transactions involving 2,264 target firms.

Figure 1.2 provides information on the number of M&As per year, demonstrating that there is a decline in M&A activity during the financial crisis.

1.2.1.2 RAIS Data

Information on linked employer-employee relationships is obtained from RAIS that is collected by the Brazilian Ministry of Labor (Ministério de Trabalho e Emprego - MTE) since 1976. RAIS is a longitudinal comprehensive administrative dataset that is compiled at an annual basis from information collected directly by formally-registered, public or private firms, and includes labor contracts that were active for at least part of the previous calendar year. The aim of the RAIS dataset is to administer and monitor access to unemployment insurance and payment of benefits to eligible employees, and, therefore, firms have strong incentives to provide comprehensive and accurate information in MTE. In addition, control mechanisms are in place to ensure mandatory compliance to the requirements of RAIS. Based on estimates of the Ministry of Labor, RAIS includes over 95% percent of formally-employed individuals in Brazil.

The unit of observation in RAIS is a job entry that is identified by an employee-level identifier (PIS) and a plant-level identifier (CNPJ) that enable me to track individuals over time and across firms. The firm-level identifier is extracted in a systematic manner by the plant-level identifier and is used to merge RAIS with the information collected from Thomson's SDC on the targets' and acquirers' firm identifiers. In addition, RAIS includes information regarding start and ending dates of employment, occupation type, wage level, and demographic characteristics. The ending date is available in a given year if the employee was separated from the job in the specific year. The occupation type is coded according to the Classificação Brasileira de Ocupaçções (CBO). RAIS contains a CBO code based on the 1994 classification and one based on the 2002 classification. Following the approach developed by Muendler, Poole, Ramey, and Wajnberg (2004), the 1994 CBO codes are mapped to the International Standard Classification of Occupations (ISCO).

At the plant-level, RAIS contains information on the geographical location of the plant, and the sector that the specific plant operates. At the individual-level, available demographic characteristics include gender, age, and education level. Although the data are collected on an annual basis, the structure allows me to retrospectively analyze at a monthly frequency utilizing the information on starting and ending month of the employer-employee relationship. I restrict my sample to the years from 2002 to 2014 so that employee-level information of at least 2 years before and after the M&A transaction is included.

The cases that an individual is reported multiple times in a specific year and the action that I have followed are presented below:

- The individual is concurrently employed at multiple firms. Following Muendler and Rauch (2011), the job entry with the earliest hiring date and the highest wage is selected.

- The employment contract of an individual with a firm is terminated in a given year and the individual is later hired by a firm. The job entry that responds to employment at the month of interest is used for the specific year.

- The individual is transferred in a different plant of the firm. As my analysis is at the firm-level and to preserve the succession nature of internal transfers, the latest job entry of the specific year is used.

- The individual performs multiple occupation types in a specific firm throughout the year. Following Muendler and Rauch (2011), the job entry with the earliest hiring date and the highest wage is selected.

I restrict my sample to workers with an age ranging from 16 to 55 so as to mitigate concerns regarding any potential impact of early retirement on entry and exit of workers in the data.[1]

1.2.1.3 Routine Task Intensity Data

Information on the level of routine intensity of a specific occupational category is provided by Autor and Dorn (2013). Autor and Dorn (2013) create an index that measures the occupational routine task intensity (RTI) based on the types of tasks (abstract, manual, routine) performed in a given occupational category. I take advantage of the crosswalk path provided by Autor and Dorn (2013) that assigns RTI scores to Census Occupational Codes (OCC1990DD), to perform a mapping of the CBO codes found in RAIS with RTI scores.

I consider an individual to perform a routine occupation if the RTI score is positive and a non-routine occupation if the RTI score is negative. Alternatively, an occupation is defined to be routine-task intensive if the occupation is in the top employment-weighted third of routine-task intensity in a given year. The results are unchanged regardless of the definition of routine workers (the correlation of the routine variables is 0.89).

[1] I use the same age restriction as Bustos, Caprettini, and Ponticelli (2016a).

1.2.1.4 Final Sample

My sample represents the intersection between Thomson's SDC and RAIS including in total 2,058 M&A transactions involving 2,264 target firms. Figure 1.3 compares the share of target firms by size category at the time of the transaction with the correspondent share of the population of firms present in my data and not involved in M&A activity (around 5 million). I use the categorization used by the Brazilian National Statistical Institute (IBGE), which is based on number of employees to sort firms in four size categories. The IBGE defines as *Micro* firms that have between 1 and 9 employees, *Small* that have between 10 and 49 employees *Medium* that have between 50 and 99 employees, and *Large* firms with 100 or more employees. As shown, target firms are on average larger. In particular, around 95% of target firms have at least 10 employees compared to only 16.5% in the population. Furthermore, Panel A of Table 1.1 demonstrates that target firms are not representative of the population of firms in terms of employment-related characteristics.

In Panel B of Table 1.1 I provide information on firm-level employment-related variables for target and acquiring firms at the time of the transaction. Relative to target firms, acquiring firms are on average three times larger in terms of total employment and have a higher share of high-skilled and non-routine human capital at the time of the M&A transaction.

Panel C of Table 1.1 provides information on the deal characteristics. Notice that only 4% of the firms targeted at an M&A transaction in Brazil are publicly listed, while 40% of the M&A activity is cross-border demonstrating the increased interest in the Brazilian economy from foreign investors.

In terms of the sector targeted at M&A activity, Table 1.2 provides information on the sectors affected by the merger activity in Brazil. I base my sector information on the 2-digit CNAE classification found in RAIS. M&A activity in Brazil has been largely driven by the commodity boom and followed domestic consumption patterns being concentrated on activities related to Business Services, IT and the Food and Beverage Manufacturing.

1.2.2 Empirical Methodology

For the purposes of my empirical analysis, I follow previous studies in the M&A literature that have used micro data at the plant level (e.g. Maksimovic, Phillips, and Prabhala (2011); Li (2013); Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014)) and hypothesize that M&As represent a shock to the target firm. Thus, I use the target firm as the unit of analysis. I employ a difference-in-differences approach around the timing of the M&A transaction to examine the impact of M&A activity on labor-related outcomes of target firms. I focus on a four-year window around the timing of the M&A transaction. In selecting the timing of the M&A, I use the effective date that the M&A transaction occurred provided by Thomson's SDC. However, the assignment of firms as targets of M&A activity is not random. Therefore, the primary econometric concern is selection bias reflecting the non-random nature of the distribution of M&A activity across industries and firm characteristics. Indeed, target firms in my sample are disproportionally larger than the average firm and are concentrated in specific industries. In order to address the potential selection issue, I allow for nonlinear and nonparametric methods with the use of matching estimators. The rationale of a matching estimator approach is to achieve optimal matching of treated firms with control firms based on multiple observable characteristics so as to restrict the set of counterfactuals to the matched controls, or in other words, identify a set of control firms that are expected to follow a similar path to the treatment group in the absence of treatment. A plausibly causal interpretation of the estimated treatment effect is based on the parallel trend assumption. The parallel trend assumption requires that the treated and control groups would have followed parallel trends in the absence of treatment. As the counterfactual outcomes are unobservable, I assess the plausibility of the assumption by comparing pre-trends of my variables. In my empirical analysis, I present results from a dynamic difference-in-differences specification that shows that treated and control firms follow parallel paths in the pre-takeover period. To reinforce the M&A-related interpretation of the results, I augment the empirical analysis by documenting that there are no employment effects on withdrawn M&As for reasons unrelated to labor considerations.

To construct the set of counterfactual firms, I closely follow Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014) and take advantage of the large number of firms in RAIS so as to control for a set of interactions among size, industry, business type, multi-establishment status, productivity and year of the transaction. Specifically, target firms are sorted into cells defined by the cross-product of the aforementioned characteristics at the time of the transaction and matched with a set of control firms that fall into the same cell and have never been involved in M&A activity either as a target or as an acquirer. In order to end up with a manageable sample for my employee-level specifications, I restrict the number of control firms to be at most 20 firms by ranking the set of potential control firms based on the absolute difference of total employment between the control and the treated firm and selecting at most the first 20 firms. To empirically categorize firms into productivity types, I apply the methodology of Abowd, Kramarz, and Margolis (1999) and instrument for productivity by using the distribution of the firm-specific wage premium. The definition of potential high-productivity firms as high-wage firms is consistent with numerous recent models of frictional labor markets (e.g., Christensen, Lentz, Mortensen, Neumann, and Werwatz (2005)), in which higher-productivity firms pay higher wages for equivalent workers.

My empirical work aims to characterize the extent and direction of post-takeover reorganization of human capital to document the decisions that acquiring firms make in readjusting the boundaries of the firm. I begin my empirical analysis by examining the impact of M&As on total employment and total wage bill of target firms to demonstrate the extent of post-takeover restructuring. I continue by exploring the sources of this adjustment in employment. To this end, I focus on the rates of firing, hiring, and voluntary separations, and the number of occupations. In a third step, I investigate how this adjustment affects different groups of workers using worker-level data. The outcomes of interest in the worker-level analysis involve binary variables that take the value of 1 in case there is a specific type of separation from the firm. I define a worker's separation as a quit (voluntary) or a layoff (involuntary) from the last formal employment in the calendar year. When I infer separations, I exclude transfers across plants within the same firm, as well as retirements and reported deaths on the job. The disaggregate nature of the separation variable is particularly useful as it allows me to disentangle involuntary from voluntary separations. Furthermore, in transactions where the information on the acquiring firm is available, I consider employees of the target firms that transfer to the acquirer as an internal transfer. Finally, I return to the firm-level analysis to examine whether changes in the demand for different employee groups induced by takeovers lead to firm-level changes in the composition of labor, average wages and measures capturing the level of within-firm wage inequality. In addition, I test whether different M&A types lead to a more rigorous labor restructuring process.

1.2.2.1 Estimation of the AKM Wage Model and Productivity Measure

Abowd, Kramarz, and Margolis (1999) provide a methodology to quantify the relative importance of worker versus firm components in determining wages. Using longitudinal matched employer-employee datasets, AKM estimates a wage regression that decomposes log-wages in a person and a firm fixed effect. Formally, following Abowd, Kramarz, and Margolis (1999), I specify a loglinear statistical model of wages as follows:

$$w_{iJ(i,t)} = \theta_i + X_{it}^{'}\beta + \psi_{J(i,t)} + \mu_t + \varepsilon_{it}, \tag{1.1}$$

where J(i, t) is the firm for which person i works at time t. The person fixed effect, θ_i, captures the contribution of unobservable time-invariant individual characteristics on wages. $X_{it}^{'}\beta$ captures the effect of person-specific time-varying factors. I include age, education and gender. The term $\psi_{J(i,t)}$ captures the effect of unobservable time-invariant firm characteristics on wages of all employees of firm j (all i with $J(i,t) = j$). Finally, ε_{it} is the error term. The AKM model assumes that the assignment of employees to firms is uncorrelated with the error term. To identify the person and firm fixed effects, the AKM specification requires the presence of employees that switch firms in matched employer-employee datasets. In the absence of movers, separating the effect of individual from firm effects would be improbable. Yet the presence of movers does not guarantee identification of all fixed effects. AKM provides an algorithm that exploits mobility to construct sets of firms and employees whose fixed effects are identifiable (the "connected set"). In typical matched employer-employee datasets, the largest group comprises consists of over 95% of the observations. Thus, restricting attention to the largest group of the sample is not a significant limitation.

I use the distribution of the firm-specific fixed effect extracted from the AKM methodology to instrument for productivity of firms. The definition of potential high-productivity firms as high-wage firms is consistent with numerous recent models of frictional labor markets (e.g., Christensen, Lentz, Mortensen, Neumann, and Werwatz (2005)), in which higher-productivity firms pay higher wages for equivalent workers, and has been extensively used in empirical studies (e.g. Serafinelli (2017)). Notice that the productivity measure that I use is time-invariant and thus, I cannot track changes in productivity in the post-takeover period.

I closely follow Lopes de Melo (2018) to apply the AKM methodology to RAIS data. Specifically, I restrict my focus to full-time employees within an age range from 16 to 55 that have at least completed basic education for the 12-year period from 2002 to 2014. To account for informality effects, I exclude individuals with available information for less than 5 of the 12 years, and who have been employed in more than three firms per year. The largest connected set includes 98.2% of the sample. Then, I categorize firms into three productivity types based on the distribution of the firm-specific fixed effect component.

1.2.2.2 Summary Statistics

Table 1.3 presents firm- and worker-level summary statistics for treated and control firms. Panel A reports firm-level employment-related characteristics for the pre-takeover period and documents that control and treated firms are similar in terms of total employment, total wage bill and the human capital structure. Panel B and C of Table 1.3 present firm-level and worker-level descriptive statistics as observed at the time of the M&A transaction. In Panel B, I report firm-level summary statistics based on 2,204 unique treated firms and 20,257 control firms. As shown, treated firms and control firms are similar at the time of the M&A event. This is consistent with my matching procedure finding similar counterfactuals. In Panel C, I report summary statistics at worker-level, which are based on 2,281,039 unique workers of treated firms and 9,021,397 unique workers of control firms. As shown, workers display similar characteristics in terms of education, gender, age, tenure and average log wage.

1.3 Results

1.3.1 Firm-Level Analysis of Total Employment and Total Wages

The objective of this section is to document the main effects of being targeted at a takeover on employment-related outcomes. To this end, the firm is used as the unit of analysis and the following difference-in-differences empirical specification is employed:

$$Y_{itpm} = \alpha_i + \alpha_{mt} + \gamma Post_p + \beta Post_p \times I_i^{treated} + \varepsilon_{itpm} \tag{1.2}$$

where i indexes firms, t indexes the calendar year, p indexes normalized time expressed in years around the M&A transaction ranging from -2 to +2 and m indexes municipalities. $Post_p$ is a dummy equal to 1 for the two-year period following the M&A transaction, and zero for the the two-year period prior to the M&A transaction. Finally, the variable $I_i^{treated}$ is an indicator function equal to 1 for firms that have been targeted in an M&A transaction at any point in time, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. The coefficient of interest is β, which captures the average difference in the outcome variable between treated and control firms in the two-year period after the M&A transaction relative to the period before the transaction. I employ firm and municipality $\times$ year. The standard errors are clustered at the transaction level so as to account for correlation between the different target firms that are involved in an M&A transaction.

I begin my analysis by examining the evolution of total employment in target firms following the M&A transaction. Specifically, I measure total firm-level employment as the logarithm of the number of employees in a firm. In case M&As are motivated by a value-maximizing perspective in the pursuit of synergy gains through consolidation and cost-reduction, human capital restructuring is expected to be extensive involving a downsizing in employment. The results of estimating equation (1.2) are reported in Column (1) of Table 1.4. As shown in Column (1), treated firms experience a statistically significant decline in the level of employment with respect to comparable firms never engaged in M&A activity in the period under study. The magnitude of my estimates indicate that target firms experience, on average, a decline of 29% in total employment. In particular, the employment level of the median target firm reduces from 139 employees at the time of the takeover to 96 in the post-merger period.

I next explore the evolution of total wage bill in target firms after the M&A transaction. I expect the decline in employment in the post-takeover period for target firms to be followed by a decline in the wage bill. Indeed, Column (2) of Table 1.4 reports the results on total wages. The findings in Column (2) demonstrate that the decrease of total wages in target firms is 17% larger than in control firms.

Finally, I further complement the firm-level analysis by focusing on the direction of the post-merger level of wage per worker. The results are reported in Column (3) of Table 1.4. Interestingly, as shown in column (3), I find that wage per worker exhibits on average an increase of 14% for target firms in the post-merger period, implying the existence of changes in the composition of labor leading to an increase in the wage level of the remaining and/or incoming employees, or both.

In order to provide further insight into the evolution of employment and wage outcomes, I estimate firm-level dynamic effects of M&As by employing the following non-parametric event-study specification:

$$Y_{itpm} = \alpha_i + \alpha_{mt} + \sum_{p=-2}^{p=+2} \gamma_p(T_p) + \sum_{p=-2}^{p=+2} \beta_p T_p \times (I_i^{treated}) + \varepsilon_{itpm} \tag{1.3}$$

where i indexes firms, t indexes the calendar year, p indexes normalized time expressed in years around the M&A transaction ranging from -2 to +2 and m indexes municipalities. T_p is a dummy equal to 1 if $T_p = p$. Finally, the variable $I_i^{treated}$ is an indicator function equal to 1 for firms that have been targeted in an M&A transaction at any point in time, and equal to 0 for control firms that are never targets or acquirers during the period under study. The coefficient of interest is β_p, which captures the average difference in the outcome variable between treated and control firms when $T_p = p$. The specification includes firm and municipality $\times$ year so as to absorb time-invariant differences across firms, municipalities and years. The standard errors are clustered at the transaction level.

The results are presented in Figures 1.4 and 1.5. As illustrated, there are no differential pre-trends between treated and control firms in terms of total employment and total wage bill up to the time of the transaction. This is expected as the purpose of the matching process is to mitigate any pre-trend differences in the matching variables. However, there is a sharp decline in employment in the two-year post-takeover period.

1.3.2 Firm-Level Analysis of Employment Flows

The net employment results demonstrate that corporate takeovers in Brazil are associated with extensive human capital restructuring. Although net employment results are informative of the direction of the post-takeover restructuring, a decline in net employment relative to the set of control firms is likely to reflect an increase in separations and/or a decline in hirings. Most importantly, an increase in separations is likely to reflect both voluntary and involuntary departures, leading to a different interpretation of the post-takeover labor-related outcomes depending on the type of separation. For example, in case M&As are motivated by cost reduction and consolidation considerations, layoffs are expected to materialize as a by-product of the restructuring process. On the other hand, M&As represent turbulent times for the employees of the target firm and thus, an increase in voluntary exit of human capital is likely to lead to a decline in employment even if the motivation of the takeover is unrelated to the pursuit of efficiency gains. To shed light on the sources of the adjustment in net employment, I transition my focus on employment flows. An important advantage of my administrative dataset is that it contains unique information on the terminations of labor contracts which allows me to disentangle involuntary from voluntary separations and, thus, document the precise manner that employment adjustment takes place. Voluntary separation refers to employee-induced terminations (e.g. resignations), while involuntary separation refers to employer-induced terminations including both layoffs and fixed-contract terminations without a subsequent renewal. In inferring separations, I exclude within-firm transfers, as well as retirements and reported deaths. Furthermore, in transactions where the information on the acquiring firm is available, I consider any post-takeover employment flows that involve the acquiring and the target firm as an internal transfer. In addition, I exploit information on occupational codes to create a variable that captures the number of occupations at the firm level as an additional variable of interest. Specifically, examining changes in the number of occupations is informative of restructuring actions related to consolidation.

For the purposes of my analysis, I take advantage of the firm-level empirical specification of the previous section. The variables of interest that refer to employment-related flows are divided by total employment. The results are reported in Table 1.5.

In column (1), I focus on total separations disregarding information on whether the separation has been initiated by the employer or the employee, and document that there is a statistically significant positive relation between being targeted at an M&A transaction and the rate of separations. The magnitudes of my estimates indicate that target firms experience an increase of 38% in separation rates in the post-takeover period compared to firms never involved in M&A activity in the period under examination. Columns (2) and (3) delve deeper in the direction of separations, and document that corporate takeovers are primarily associated with an increase in the rate of involuntary separations. Indeed, there is a statistically significant increase in layoffs by 32.6%, while the change in the rate of voluntary separations significantly increases by 5.4%. Specifically, in absolute terms, the median target firm experiences a decline in the number of employees, 75% of which is due to layoffs and 25% is due to voluntary exit. In Column (4) I transition my focus on the post-takeover hiring rates to document that increased post-takeover layoffs are also accompanied by limited hiring activity. Indeed, target firms decrease hiring rates by 32% compared to control firms.

Therefore, acquiring firms predominantly reorganize the labor force in the target firm by increasing layoffs and limiting hirings, consistent with the notion that M&As engage in efficiency-seeking consolidation; however, at the same they experience an increased rate of voluntary exit of human capital. I further supplement my findings by exploring the impact of M&As on the number of occupations present in target firms. In particular, the observed increase in layoffs and the decline in hirings is likely to reflect occupational consolidation, implying that layoffs are accompanied by non-replacement of redundant workers, consistent with firms consolidating or limiting unrelated occupational tasks. Column (5) reports the results demonstrating that target firms are associated with a 11% decrease in the number of occupations in the post-takeover period.

1.3.3 Worker-Level Analysis on Labor Restructuring

Having established firm-level patterns in the labor flows of target firms, I turn to an employee-level analysis to examine post-takeover changes in the demand for human capital. To the extent that acquiring firms actively reorganize human capital in the process of resetting their boundaries in the post-takeover period, I should expect M&As to be followed by heterogeneous effects on different groups of employees leading to changes in the composition of labor. For this purpose, I exploit cross-sectional variation in worker characteristics and characterize the post-takeover change in the likelihood of exit for different groups of employees. To separate demand and supply side factors that affect the decision to exit the firm, I use the information on the reason for the termination of employment contracts and disentangle involuntary from voluntary exit. This cross-sectional analysis extends our understanding of the post-takeover readjustment process by linking heterogeneous post-merger outcomes on different groups of employees with theoretical predictions emerging from theories that attempt to explain how M&As emerge and what are the effects on firms.

For the purposes of the worker-level analysis, the individual is used as the unit of analysis and the empirical specification has the following form:

$$Y_{ijptm} = \alpha_j + \alpha_m + \alpha_t + \gamma Post_p + \delta I_j^{treated} + \beta(I_j^{treated} \times Post_p) + X_j^{'}\theta + \varepsilon_{ijptm} \tag{1.4}$$

where i indexes firms, j indexes workers, p indexes normalized time expressed in years around the M&A transaction ranging from -2 to +2, t indexes the calendar year and m indexes municipalities. The variable $I_j^{treated}$ is an indicator function equal to 1 for individuals that have been employed in a firm i that was the target of an M&A transaction at any point in time, and equal to 0 for the individuals of control firms. Finally, $Post_p$ is a dummy equal to 1 for the two-year period following the M&A transaction, and zero for the the two-year period prior to the M&A transaction. The coefficient of interest is β, which captures the average difference in the outcome variable between treated and control firms in the two-year period after the M&A transaction relative to the period before the transaction. X_j' is a vector of individual controls that includes age, gender, education and tenure. The specification includes employ employee, municipality and year fixed effects. The standard errors are clustered at the transaction level. The dependent variable is a binary variable denoting the occurrence of specific type of separation from the firm. The type of separation includes voluntary departure from the firm and involuntary separation that includes dismissals or fixed contract terminations without renewal.

The selection of the employee-level variation that I explore is motivated by theoretical predictions about the effects of M&As on firms. Specifically, I focus on the following human capital dimensions that are relevant to the reorganization decision of the acquiring firm: level of skill, employees in managerial positions, occupational routine task intensity, and level of human capital overlap. The neoclassical merger theory is based on the view that M&As are an efficient response to regime shifts (e.g. due to technological shocks) by value-maximizing managers (e.g. Gort (1969); Mitchell and Mulherin (1996); Jovanovic, Rousseau, et al. (2001)). Therefore, takeovers potentially lead to technological change and adoption of automation, and to the extent that capital and high-skilled labor are complements, these theories would predict that M&As should generate an increase in the demand for high-skilled and non-routine labor. In addition, synergy gains have long been considered as an important driver of M&As (e.g. Andrade, Mitchell, and Stafford (2001); Devos, Kadapakkam, and Krishnamurthy (2008)). An important channel though which synergies materialize is the existence of asset complementarities between the target and the acquiring firm (e.g. Rhodes-Kropf and Robinson (2008)). Therefore, these theories would predict an increase in the likelihood of involuntary separation for workers in occupations that overlap between the acquiring and the target firm.

1.3.3.1 High-Skilled and Low-Skilled Labor

I begin my worker-level analysis by exploring the post-merger demand for high-skilled and low-skilled workers. High-skilled workers are considered to be a scarce and redeployable resource, instrumental for firm productivity and value creation (Abowd, Haltiwanger, Jarmin, Lane, Lengermann, McCue, McKinney, and Sandusky (2005)). Thus, I expect acquiring firms to attempt to retain the target's high-skilled employees. On the other hand, in case M&As are motivated by cost reduction and consolidation considerations, layoffs of high-skilled workers are likely to materialize as a by-product of the consolidation process. In addition, M&As are a source of disruption for target firms associated with an increase in uncertainty related to employment prospects, and given the highly liquid market for high-skilled employees, I expect M&As to generate an increased likelihood in voluntary exit for high-skilled workers. On the contrary, given the abundance of low-skilled labor in Brazil and the lower labor adjustment costs, the role of low-skilled employees in the restructuring process is expected to be less pronounced - if any.

I proxy for skill by taking advantage of information on the educational level of employees. Specifically, I define as high-skilled any employee that has completed at least undergraduate education, while I define as low-skilled any employee having completed at most high-school education. I begin my analysis by examining involuntary separations. The dependent variable is a binary variable that takes the value of 1 for the year that an employee experiences employer-induced separation and 0 otherwise. The results are reported in Column (1) and (3) of Table 1.6. Column (1) focuses on the subsample of high-skilled labor, while column (3) examines the impact on low-skilled labor. As demonstrated, acquiring firms attempt to retain the target's high-skilled labor, as the likelihood of involuntary separation in the post-takeover period is not statistically significant. On the contrary, low-skilled labor is particularly affected by labor restructuring experiencing a higher likelihood of involuntary separation following an M&A transaction. The magnitudes of my estimates indicate that low-skilled labor is associated with an increase of 5.9% in the likelihood of involuntary separation. Relative to the average effect of 16.9%, the estimate implies that low-skilled employees are 35% more likely to be fired in the post-merger period. This implies that the observed post-merger increase in layoffs hits low-skilled workers disproportionally.

Given that M&As induce occupational uncertainty, the post-takeover period represents uncharted territory for the labor force of target firms, potentially leading to voluntary exit. Therefore, in Columns (2) and (4) of Table 1.6, I focus my attention to voluntary separations to explore the heterogeneous response of high- and low-skilled employees. High-skilled labor, faced with a better outside option, has the opportunity to exploit the highly liquid labor market and depart from the target firm after the takeover. On the contrary, low-skilled is less likely to exhibit any sensitivity to the merger shock. Indeed, Column (2) demonstrates that treated firms face a 1% increase in the likelihood of high-skilled employees exiting the target firm; however, as shown in column (4), there is no effect for low-skilled labor. In terms of magnitude, relative to the average likelihood of voluntary separation of 6.8%, high-skilled employees exhibit a 15% increase in the likelihood of voluntary exit in the post-takeover.

1.3.3.2 Employees in Managerial Positions

Next, I examine the reorganization decisions of acquiring firms regarding employees in managerial positions. Predicting the impact of M&As on managers is less straightforward. On the one hand, acquiring firms may have the incentive to replace managers so as to appoint their own in an effort to instill their management practices and corporate culture. In addition, managers may be deemed redundant as part of the consolidation process. On the other hand, retaining the managers of the target firms may be beneficial for the integration process due to deeper knowledge of the internal processes of the target firm. Therefore, it is unclear what is expected to be the direction of the reorganization process. I argue that cross-border M&As represent a subsample where the benefits of maintaining the managers of the target firm is beneficial. Specifically, expanding in a new geographical market is followed by increased uncertainty, and managerial knowledge on local business practices may be beneficial to alleviate such concerns.

I identify managerial positions by exploiting information on the occupational code of each employee. Following the approach developed by Muendler, Poole, Ramey, and Wajnberg (2004), I map CBO codes to International Standard Classification of Occupations (ISCO) codes, and I exploit the ISCO-88 codes of the employees to identify managers. The results are reported in 1.7. Columns (1) to (3) focus on the likelihood of involuntary separation. I find that, unlike the rest of the high-skilled employees, managers experience an increase in the post-takeover likelihood of involuntary separation in transactions that involve domestic firms. The point estimates indicate a 4% increase in the likelihood of involuntary separation for managers in domestic M&As. This finding is in line with the Jensen and Ruback (1983) view that takeovers induce competition for the right to manage resources and achieve efficiency by replacing managers in target firms. On the contrary, cross-border acquirers appear to retain the managers of target firms consistent with the notion that cross-border acquirers benefit from managerial knowledge of both the local business environment and the internal processes of the target firm. Interestingly, I exploit information on the nationality of employees and I observe that in 26% of cross-border transactions, acquirers appoint a foreign manager potentially to supervise the process in an effort to instill the corporate culture and apply the management practices of the acquirer. In Columns (4) to (6) I explore changes in the likelihood of voluntary exit, and find that there is no change in the likelihood of voluntary exit after the takeover.

1.3.3.3 Routine-Biased Change

The neoclassical theory on M&As emphasizes the role of M&As as an efficient response to regime shifts, generating technological change (e.g. Jovanovic, Rousseau, et al. (2001), Jovanovic and Rousseau (2008)). In addition, Ma, Ouimet, and Simintzi (2017) link industry-level intensity in M&A activity with within-industry occupational shifts that reflect routine-biased change. Indeed, M&As are likely to reduce frictions associated with technology adoption and increase automation for reasons ranging from the pursuit of efficiency gains (Mitchell and Mulherin (1996)) to alleviating financial constraints (e.g. Erel, Jang, and Weisbach (2015)). To test this hypothesis, I focus on cross-border M&A transactions. Over 90% of cross-border M&A activity in Brazil involves acquiring firms from developed countries, implying that M&As are likely to constitute a vehicle of technological and organizational change. Indeed, the MNE literature has highlighted that MNEs consist of superior knowledge-based assets and possess competitive advantages transferable to the market of the host country (e.g. Hymer (1976)), while the trade literature has demonstrated the benefits of increased exposure to trade in terms of skill and technology upgrade (Verhoogen (2008); Bustos (2011)). Therefore, a Brazilian firm is likely to experience a decline in the frictions associated with technology adoption and increase in automation for various reasons including increased access to capital, transfer and upgrade of technology and greater adoption of modern management practices. In such cases, the target firm is bound to experience changes in labor demand induced by routine-biased technological change.

I exploit occupational information on routine intensity by Autor and Dorn (2013). An employee is considered to perform a routine occupation if the routine task intensity (RTI) score is positive and a non-routine occupation if the RTI score is negative. Alternatively, an occupation is defined to be routine-task intensive if the occupation is in the top employment-weighted third of routine-task intensity in a given year. The results are unchanged regardless of the definition of routine workers (the correlation of the routine variables is 0.89).

The results are presented in Table 1.8. Columns (1) and (4) report the results for the full sample of M&As in Brazil and document that there is a post-takeover increase in the likelihood of involuntary separation for both routine and non-routine employees. Specifically, the magnitudes of my estimates indicate that workers performing non-routine tasks are associated with a 2.7% increase in the likelihood of involuntary separation, while routine workers experience a 6% increase.

Nevertheless, in Columns (3) and (6), I repeat the analysis for the subsample of cross-border M&As, where I argue that automation and routine-biased change is more likely to occur. Indeed, the results indicate that post-takeover restructuring in cross-border M&As is indicative of routine-biased change with employees that perform routine tasks experiencing a 25% increase in the likelihood of involuntary separation. Non-routine employees exhibit no change in the post-takeover likelihood of involuntary separation. On the contrary, the likelihood of involuntary separation increases for both routine and non-routine employees, though at a higher rate for routine employees. Specifically, the likelihood of involuntary separation increases by 7.1% for routine employees and 3.7% for non-routine employees.

1.3.3.4 Hirings

Next, I turn to the analysis of the reorganization decisions of target firms in terms of hirings. M&As are associated with involuntary separations of low-skilled and routine employees at the target firms and experience voluntary exit of high-skilled employees. Examining their hiring decisions is important to shed light into the incentives of the firms involved in M&A activity in terms of the direction of the composition of human capital. To analyze the hiring decisions at target firms, I estimate specification (1.5) using as dependent variable an indicator that takes the value of 1 in the period an employee is hired by the firm and 0 otherwise.

The results are presented in Table 1.9. Columns (1) and (2) report the results for low-skilled and high-skilled human capital, while Columns (3) and (4) examine the hiring decisions regarding routine and non-routine employees. The results document that there is a post-takeover significant decline in the hiring likelihood only for low-skilled and routine employees. Specifically, the magnitudes of my estimates indicate that low-skilled workers are associated with a 5.5% decline in the likelihood of being hired, while workers performing routine tasks experience a 4.4% decline. Combined with the previous findings that low-skilled and routine human capital is particularly affected in the post-takeover period through forced displacements, these results imply that acquiring firms actively aim at reducing the level of low-skilled employment and inducing automation at the target firms. On the contrary, there is no decline in the hiring rate of high-skilled and non-routine employees, suggesting that target firms actively decide to alter the composition of human capital and operate with a larger share of high-skilled and non-routine labor.

1.3.3.5 Occupational Overlap

In neoclassical M&A theories, asset and product-market complementarities motivate corporate takeovers (Rhodes-Kropf and Robinson (2008); Hoberg and Phillips (2010)) due to an increase in the potential for synergy gains. Lee, Mauer, and Xu (2017) empirically extend these theories to human capital by constructing an industry-level measure of human capital relatedness to demonstrate that human capital complementarities also motivate M&As.

This evidence suggests that human capital overlap is likely to partially explain the observed patterns of labor restructuring. To test this hypothesis, I focus on M&A transactions where the identities of both the target and the acquiring firm are available and exploit information on the occupational profiles of the acquiring and the target firm at the time of the takeover to construct a binary variable that takes the value of 1 for occupations that are present in both firms involved in the transaction at the time of the takeover.

To perform my empirical analysis, I restrict my sample to the employees of target and acquiring firms in the post-takeover period and employ the empirical specification presented below.

$$Y_{jptm} = \alpha_j + \alpha_m + \alpha_t + \beta I_j^{Overlap} + X_j^{'}\theta + \varepsilon_{jptm} \tag{1.5}$$

where j indexes workers, p indexes normalized time expressed in years around the M&A transaction ranging from -2 to +2, t indexes the calendar year and m indexes municipalities. The variable $I_j^{overlap}$ is an indicator function equal to 1 for individuals that perform an occupation that exists in the occupational profile of the acquiring firm at the time of the M&A transaction. Finally, $X_j^{'}$ is a vector of individual controls that includes age, gender, wage and tenure. The specification includes employee, municipality and year fixed effects. The standard errors are clustered at the transaction level. The dependent variable is a binary variable that takes the value of 1 for the occurrence of a specific type of separation from the firm at a specific point in time. The type of separation includes voluntary departure from the firm and involuntary separation either in the form of a contract termination without just cause or a fixed contract termination without renewal. Notice that the specification does not use a control group.

The results are reported in Table 1.10. Columns (1)-(3) report the results for the employees of the target firms in the post-takeover period, while Columns (4)-(6) report the results for the employees of the acquiring firms. In Columns (1) and (4) the dependent variable is a binary variable that captures voluntary separations, while in Columns (2), (3), (5) and (6) the dependent variable is a binary variable that takes the value of 1 in the case of an involuntary separation. The results demonstrate that the likelihood of involuntary separation increases for target employees with an overlapping occupation by 1.5% to 1.8% in the post-takeover period, while there is no effect for employees in acquiring firms. In addition, occupational overlap has no effect in the likelihood of voluntary separations. Columns (3) and (6) present the results of introducing a variable that takes the value of 1 low-skilled employees and the interaction with human capital overlap variable, demonstrating that low-skilled employees in target firms in overlapping occupations are disproportionally affected. This evidence suggests that occupational overlap is a key channel of increased layoffs in target firms in the post-takeover reorganization process, consistent with consolidation and cost-reduction being one of the primary drivers of takeovers.

1.3.4 Technological Change and Automation

The employee-level results indicate that takeovers lead to an increase in the relative demand for high-skilled and non-routine labor in the post-takeover period. Therefore, I turn to firm-level analysis to further document whether these changes in the demand for different groups of employees lead to firm-level changes in the composition of labor. To perform the empirical analysis, I use the firm-level empirical specification (1.2) with the dependent variable being firm-level labor shares of different groups of employees. The employee groups that I consider are the share of high-skilled employees, the share of routine employees and the share of employees performing R&D-related tasks.

The results are reported in Table 1.11 and are suggestive of skill-biased and routine-biased compositional changes in line with the employee-level results in changes in the relative demand for different groups of employees. In Column (1) the dependent variable is the share of high-skilled labor at the firm level, while in Column (2) the dependent variable is the firm-level share of routine labor. The results demonstrate an increase in the share of high-skilled labor of 7% and a decline in the share of routine labor by 7.6%.

In order to provide additional evidence of technological change and investment in technology, I explore whether there is an increase in the share of employees that perform R&D-related tasks. To this end, I exploit the detailed nature of the occupational classification in my dataset and identify the employees that are occupied in positions with R&D being the primary task. Column (3) of Table 1.11 presents the results, showing that there is a statistically significant post-takeover increase by 1.2% in the firm-level share of employees in occupational tasks related to R&D.

1.3.5 Average Wages and Wage Inequality

My results demonstrate an increase in the relative demand for high-skilled and non-routine labor that are reflected to firm-level changes in the composition of labor after the takeover. These changes are likely to disproportionally affect the average wages of high-skilled and low-skilled employees. I find that this is indeed the case. In Column (1) and (2) of Table 1.12 I use the firm-level empirical specification (1.2) with firm-level average wages as the dependent variable, to document the impact of corporate takeovers on average wages. As reported in Column (2), M&As are associated with a decline in average wages for low-skilled workers; however, as shown in Column (1), high-skilled employees experience an increase in the average wage by 4.9% in the post-takeover period.

In addition, Autor and Dorn (2013) document that routine-intensive occupations are concentrated in the middle of the distribution of skill, implying that job polarization is accompanied by wage polarization. Therefore, I expect that job polarization and the relative increase in labor demand for skilled workers in the post-takeover period are expected to contribute to an increase in within-firm wage inequality in target firms.

To perform the analysis, I use the firm-level empirical specification (1.6) presented below.

$$\text{Inequality}_{itpm} = \alpha_i + \alpha_{mt} + \gamma Post_p + \beta Post_p \times I_i^{treated} + \varepsilon_{itpm} \tag{1.6}$$

The measures of wage inequality that I use as dependent variables are the standard deviation of (log) wages and the ratio of the 90th wage percentile to the 10th wage percentile. The results are reported in Table 1.13 and document a strong statistically significant positive relationship in the post-takeover period. Column (1) focuses on the the standard deviation of (log) wages as the inequality measure and demonstrates that M&As are associated with an increase in wage dispersion of 6.8%, while Column (2) uses the 90-10 wage ratio and confirms that takeovers lead to an increase in within-firm wage inequality in the post-takeover period. The point estimates for the 90-10 wage ratio indicate that M&As increase the 90-10 wage inequality gap by 13.3%.

1.3.6 Combined Entity

Thus far, my results indicate that there is substantial post-takeover restructuring of the human capital at target firms that is indicative of skill-biased and routine-biased change. However, reorganization decisions in M&As are guided by labor considerations related to the human capital of the acquiring firms as well and, thus are expected to involve and affect the human capital of the combined entity. To this end, I next explore if the observed firm-level employment outcomes and changes in the composition of labor at target firms are reflected in the combined entity by analyzing changes in aggregate employment-related outcomes and the human capital profile of the combined entity. For this purpose, I repeat the firm-level analysis by considering the acquiring and the acquired firms involved in a M&A transaction as a single entity. The results are reported in Table 1.14 and, in total, demonstrate that M&As do lead to a decline in employment and labor expenses, an increase in wage inequality and do induce compositional changes in the human capital profile of the combined entity that reflect technological change and automation.

Specifically, I begin my analysis by examining the evolution of total employment and labor expenses in the combined entity following the M&A transaction. The results of estimating specification (1.2) for the combined entity are reported in Columns (1) and (2) of Table 1.14 and show that M&A firms experience a statistically significant decline in the level of employment and labor expenses with respect to comparable firms never engaged in M&A activity in the period under study. The magnitudes of my estimates indicate that the combined entity experiences, on average, a decline of 12.9% in total employment and 9.9% in total wages.

In Columns (3) and (4) of Table 1.14 I focus on the share of high-skilled and routine human capital in the combined entity in the post-takeover period and demonstrate M&As are associated with technological change and automation. Specifically, the changes in the composition of human capital point to a 4.3% increase in the share of high-skilled labor and a 4.8% decline in the share of routine employment. As shown in Column (5), these changes lead to a 9.6% post-takeover increase in within-firm wage inequality in the combined entity.

1.3.7 Takeover Types

In this section, I classify the post-takeover decisions on the reorganization of human capital by the type of the takeover. My empirical analysis is motivated by the fact that there are different predictions for the impact of M&As on the level of reorganization of assets depending on the type of the takeover. For example, focused takeovers are likely to be motivated by the potential of synergy gains through cost savings and consolidation, and therefore larger post-takeover employment losses and reductions in labor expenses are expected to materialize relative to combined pre-takeover levels compared to diversifying M&As. Therefore, I categorize M&As into focused and diversifying. For the takeovers that information on the acquiring firm is available, I follow Tate and Yang (2016) and identify a takeover as diversifying if there is no overlap in the establishment-level industries in which the acquiring and target firms operate at the time of the transaction. For the takeovers that there is no information on the acquiring firm, I use the three-digit SIC code, as reported in SDC, to classify M&As between firms in the same industry as focused. In my sample of M&As in Brazil, 83% are focused and 17% are diversifying.

For the empirical analysis, I estimate equation (1.2) separately for focused and diversifying takeovers, and examine the evolution of total employment and labor expenses in the combined entity following the M&A transaction. The results are reported in Table 1.15.

In Columns (1) and (2) I examine the impact of the type of takeover on total employment, while in Columns (3) and (4) I focus on the impact on the total wage bill. The results demonstrate that only firms involved in focused M&As experience a statistically significant decline in the level of employment and labor expenses with respect to comparable firms never engaged in M&A activity in the period under study. The magnitudes of my estimates indicate that there is a significant decline of 15.3% in total employment and 11.7% in the total wage bill for firms that engage in focused takeover. On the contrary, firms involved in diversifying M&As demonstrate no employment and wage effects.

1.3.8 Human Capital Relatedness

Lee, Mauer, and Xu (2017) empirically extend neoclassical M&A theories of asset complementarities to human capital by constructing an industry-level measure of human capital relatedness to demonstrate that human capital complementarities also motivate M&As. This evidence suggests that the level of human capital relatedness is likely to affect the level of employment adjustment in the post-takeover. To test this hypothesis, I focus on M&A transactions where the identity of both the target and the acquirer is available and exploit information on the occupational profiles of the acquiring and the target firm at the time of the takeover to construct a variable of human capital relatedness. Specifically, I follow Lee, Mauer, and Xu (2017) and construct a measure of human capital relatedness (HCR) between the acquiring firm i and the target firm j as the scalar product of the firms' occupational profile vectors divided by the product of their lengths:

$$HCR_{ij} = \frac{H_i H_j'}{\sqrt{H_i H_i' H_j H_j'}} \tag{1.7}$$

The HCR measure is bounded between 0 and 1. It is 1 for merging firms with identical occupational profiles, and 0 for firms with orthogonal human capital profiles. To perform my empirical analysis, I restrict my sample to the combined entity and employ the empirical specification (1.2). I classify takeovers into two categories based on the level of the human capital relatedness measure. The results are reported in Table 1.16.

In Columns (1) and (2) I examine the impact of human capital relatedness on total employment, while in Columns (3) and (4) I focus on the impact on the total wage bill. The results demonstrate that merging firms with higher human capital relatedness experience a larger decline in the level of employment and labor expenses compared to merging firms with a lower level of human capital relatedness. The magnitudes of my estimates indicate that there is a significant decline of 19.3% in total employment and 16.4% in the total wage bill for M&As where the firms involved have a high human capital relatedness, while in M&As where the firms involved have a low human capital relatedness, the decline in employment is 9.5% and the decline in the total wage bill is 4.1%.

1.3.9 Withdrawn M&As

The empirical methodology relies on the use of matching estimators to alleviate concerns related to selection. The assumption is that the random assignment to treatment requirement is more likely to hold within the matching cells than across the population. In an attempt to mitigate the issue of common shocks affecting both the selection of takeover targets and labor restructuring outcomes and reinforce the M&A-driven interpretation of the results, I exploit information on withdrawn M&A deals. Specifically, I identify the deals from SDC that were announced and eventually withdrawn, excluding any deals that include firms that were eventually acquired and deals that were withdrawn for labor-related reason. In particular, for each withdrawn deal, I use either news reports or the anti-trust authority report to identify the reason for the takeover withdrawal. The final sample includes 67 withdrawn M&As. I repeat the analysis for total employment and total wage bill by identifying a relevant set of control firms following the same methodological process that I used in the main empirical analysis.

Table 1.17 presents the results. I find that there is no statistically significant relationship between withdrawn M&As and total employment and total wage bill. These results provide additional evidence against concerns that the results are driven by selection into treatment.

1.3.10 Investment in Capital and Stock Market Reaction

Post-takeover restructuring is followed by an increase in the relative demand for high-skilled and non-routine labor. Since capital and high-skilled labor are complements and routine-biased change is related to investment in automation, I expect firms to increase investment in capital in the post-takeover period. Indeed, although my dataset provides no information on the financial performance of firms, I have documented a post-takeover increase in the share of labor in occupational tasks related to R&D which is indicative of an increase in firm investment in technology. To further complement my analysis, I focus on public firms that financial information is reported in Compustat to provide suggestive evidence of an increase in investment in capital. The majority of target firms in Brazil are private; however there are 82 public firms that have been targeted at M&As from 2004 to 2012. For these firms, I identify public firms in the same two-digit SIC industry that have never been involved in M&A activity and compare changes in measures of investment before and after the takeover.

Panel A of Table 1.18 provides descriptive statistics of treated and control firms, demonstrating that there are no statistically significant differences between treated and control firms. Next I turn to a multivariate regression analysis to formally test post-takeover changes in investment in capital. For the empirical analysis, I use the firm-level empirical specification (1.8) presented below.

$$\text{Investment in Capital}_{itpm} = \alpha_i + \alpha_m + \alpha_t + \gamma Post_p + \beta Post_p \times I_i^{treated} + X_i^{'}\theta + \varepsilon_{itpm} \quad (1.8)$$

The measures of investment in capital that are used as dependent variables are the ratio of capital expenditure to beginning-of-year total assets, the growth in Property, Plant and Equipment (PPE) and the growth in intangible assets. The results are reported in Panel B of Table 1.18 and document a strong positive relationship in the post-takeover period. Column (1) focuses on the growth in intangible assets as the dependent variable and demonstrates that M&As are associated with an increase in the growth of intangible assets, Column (2) uses PPE growth and confirms that takeovers lead to an increase in investment in capital in the post-takeover period, while Column (3) focuses on the ratio of capital expenditure to beginning-of-year total assets as the dependent variable and demonstrates that M&As are associated with an increase in CapEx of 2.7%.

Finally, focusing on the stock market reaction, the three-day abnormal return CAR(-1,1) is 6.37% significantly positive for the 78 publicly listed targets and 1.19% significantly positive for the 654 publicly listed acquirers, implying that the restructuring process in the post-takeover period leads to value creation.

1.3.11 Labor Restructuring and Employment Outcomes

My results demonstrate that M&As are associated with a large adjustment in employment for target firms that disproportionally affect specific types of human capital. In this section, I transition my focus on the impact of M&As on the subsequent employment outcomes of displaced employees by analyzing labor mobility in the post-takeover period for low-skilled and high-skilled employees. The RAIS dataset allows me to follow firms and employees over time and track their employments decisions. Therefore, I measure employment-related outcomes for the human capital of target firms displaced after takeovers for the two-year post-separation period.

Specifically, I begin by documenting the impact of M&As on the incidence of unemployment. For the empirical analysis, I estimate specification (1.5) using as dependent variable an indicator variable that equals 1 for employees that have experienced displacement and unemployment in the two-year period after their displacement and 0 otherwise for treated and control firms. Columns (1) and (4) in Table 1.19 present the results on unemployment incidence for low-skilled and high-skilled employees, documenting that M&As lead to a statistically significant increase in the unemployment incidence only for low-skilled employees by 3.3%. Columns (2) and (5) focus on unemployment spells as the dependent variable. Unemployment spells are estimated as the number of months of unemployment a displaced employee experiences in the two-year period after their departure from a firm in my sample. Notice that any effect on the total unemployment months combines the effect on unemployment incidence and unemployment duration. The results demonstrate that only target firms' low-skilled employees experience a significant increase in unemployment spells in the post-takeover period. The magnitudes of my estimates indicate that displaced target employees experience an increase of 25% in unemployment duration, relative to the average effect in my sample which is 2 months.

Next, I turn to job turnover by estimating the number of jobs displaced employees have had in the two-year period after their displacement. Columns (3) and (7) in Table 1.19 present the results on job turnover for low-skilled and high-skilled employees, documenting that M&As lead to a statistically significant increase in job turnover only for low-skilled employees. Relative to the average effect, displaced target employees experience an increase of 15% in the number of job changes after takeovers.

Finally, I examine the impact of M&As on the average monthly wage during a year. Notice that any treatment effects documented incorporate wage changes due to unemployment, changes in employment between firms, and changes in earnings at the current job. Columns

(4) and (8) in Table 1.19 indicate that only low-skilled employees experience a statistically significant wage decline relative to comparable employees in control firms. In total, the results in Table 1.19 confirm that low-skilled employees in target firms, on average, experience unfavorable labor outcomes in the post-takeover period.

1.4 Conclusion

I analyze the extent and direction of the labor reorganization process in firms targeted at takeovers in Brazil from 2004 to 2012. I demonstrate that corporate takeovers are associated with extensive labor restructuring at the target firm. Specifically, I find that target firms experience a large decline in total employment and total wage bill relative to control firms in the two-year post-takeover period. This adjustment in employment occurs by increasing layoffs and limiting hirings, consistent with the notion that M&As engage in efficiency-seeking consolidation. I further supplement my findings by documenting that reorganization involves occupational consolidation, as the number of occupations declines in the post-takeover period.

Having established firm-level patterns in the employment flows of target firms, I turn to an employee-level analysis to examine post-takeover changes in the demand for human capital. For this purpose, I exploit cross-sectional variation in worker characteristics and characterize the post-takeover change in the likelihood of exit for different groups of employees. To separate demand and supply side factors that affect the decision to exit the firm, I use the information on the reason for the termination of employment contracts and disentangle involuntary from voluntary exit. My findings show that the post-takeover restructuring process is indicative of skill-biased and routine-biased technological change, consistent with empirical findings that highlight the importance of high-skilled labor for firm productivity and value creation, and theoretical predictions that link M&As with automation and technological change. In addition, I provide evidence that occupational overlap is a key channel of increased layoffs.

The employee-level results indicate that takeovers lead to an increase in the relative demand for high-skilled and non-routine labor in the post-takeover period. I show that these demand changes lead to changes in the firm-level composition of labor, as the share of high-skilled labor increases by 7%, while the share of routine labor decreases by 7.6%. In order to provide additional evidence of technological change, I show that there is a post-takeover increase in the firm-level share of labor in occupational tasks related to R&D.

Finally, I focus on wages and document that average wages decline only for low-skilled labor. This heterogeneous change in wages in the post-takeover period along with the relative increase in demand for skilled and non-routine workers contribute to an increase in within-firm wage inequality.

My findings have broader implications about how acquiring firms redraw their boundaries after takeovers. Given the extent of post-takeover labor restructuring, takeovers should be viewed as a vehicle of an extensive organizational change that resets the boundaries of the firms in a manner that is indicative of technological change and efficiency-seeking consolidation.

1.5 Figures and Tables

1.5.1 Figures

Figure 1.1: Publicly Announced and Completed M&A Transactions in Brazil, Including Majority and Minority Stake Acquisition. Source: Thomson SDC

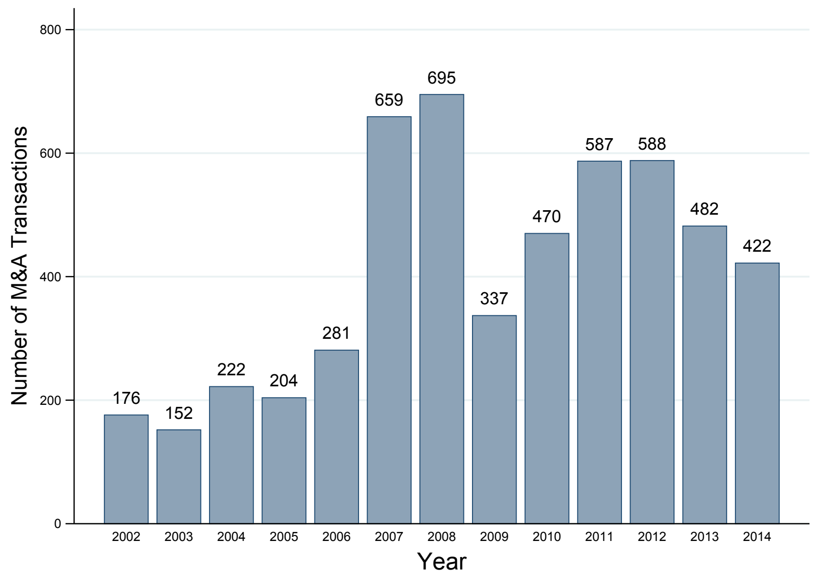


Figure 1.2: Matched Sample of Publicly Announced and Completed M&A Transactions in Brazil

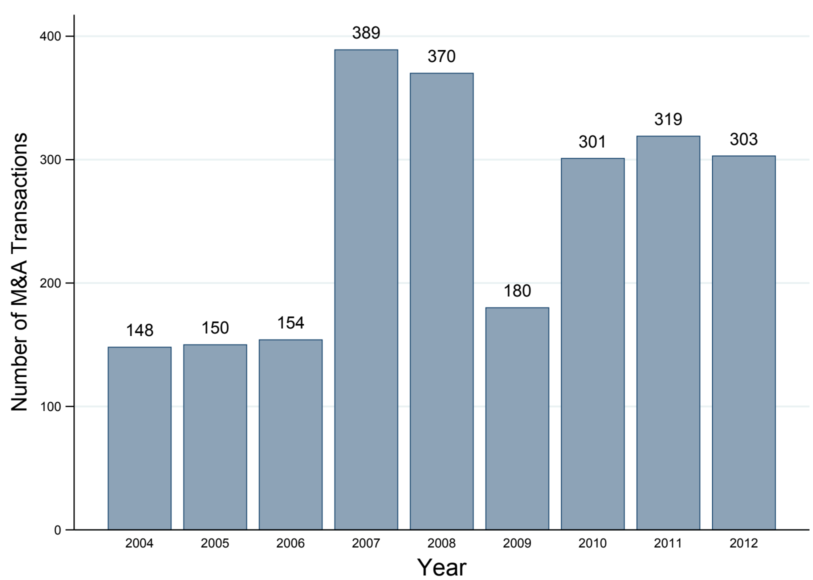


Figure 1.3: Size of Target Firms Vs. Population

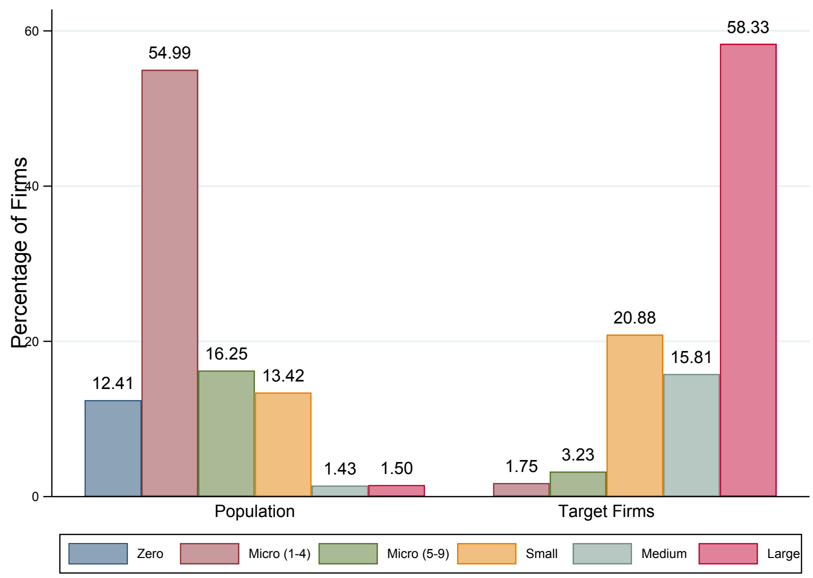


Figure 1.4: Dynamic Effects of M&A on Target's Employment Level

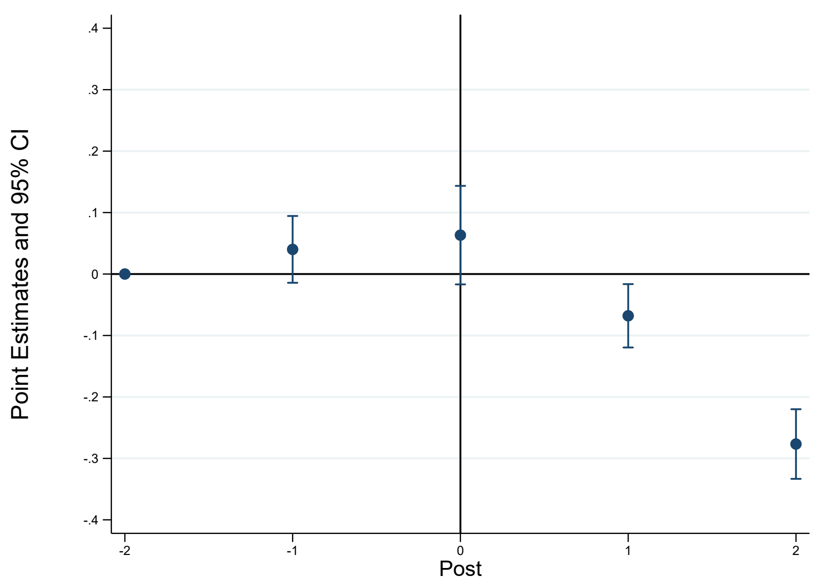
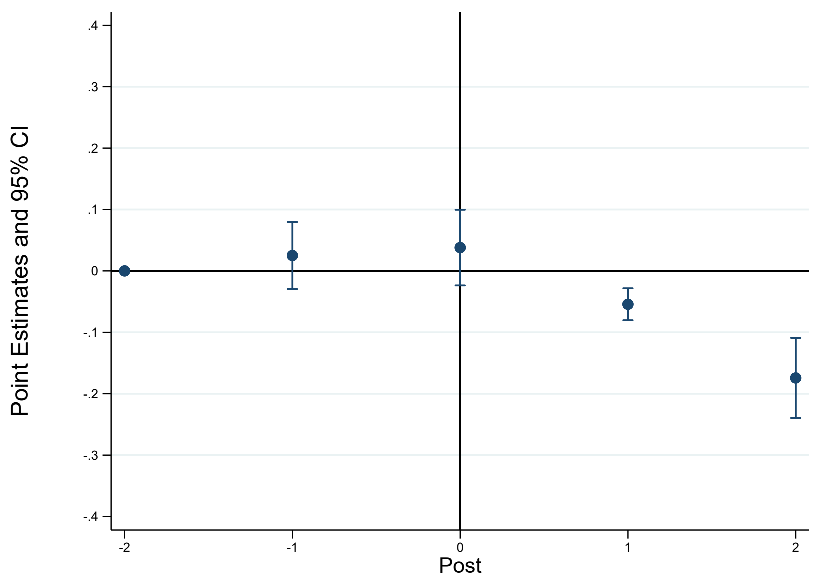


Figure 1.5: Dynamic Effects of M&A on Target's Total Wages



1.5.2 Tables

Table 1.1: Summary Statistics - M&As

	Panel A: Firm Characteristics					
	Treated Firms			**Population**		
Variables	**p50**	**Mean**	**Std Dev.**	**p50**	**Mean**	**Std Dev.**
Number of Employees	139	678	1,672	2	15	894
Total Wage Bill (R$)	231,753	1,301,086	4,135,980	1,568	19,759	1,184,941
Log Employment	4.9	4.9	1.7	1.1	1.4	1.1
Log Total Wage Bill	12	12	1.9	7.4	6.8	2.9
Number of Firms	2,204			5,056,407		

	Panel B: Target Vs. Acquirer Characteristics					
	Target Firms			**Acquiring Firms**		
Variables	**p50**	**Mean**	**Std Dev.**	**p50**	**Mean**	**Std Dev.**
Number of Employees	142	648	2,636	396	2,602	5,203
Total Wage Bill (R$)	268,153	1,265,115	5,264,600	904,683	6,352,138	26,095,079
Log Employment	5	5	1.7	6	5.8	2.3
Log Total Wage Bill	12	12	1.8	14	13	3.4
Routine Share	0.79	0.71	0.24	0.72	0.64	0.25
High-Skilled Share	0.16	0.27	0.26	0.26	0.35	0.29
Number of Firms	1,564			1,381		

	Panel C: Deal Characteristics		
Variables	**N**	**Mean**	**Std Dev.**
Deal Value ($M)	812	229.23	709.01
Cross-Border	2,058	0.40	0.49
Public Acquirer	2,058	0.35	0.48
Public Target	2,264	0.04	0.18
Diversifying	2,058	0.17	0.38
Friendly	2,058	0.98	0.15

Notes: The table reports firm-level descriptive statistics for treated firms and the population of firms. The data refer to the time of the M&A Transaction.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.2: M&A Target Firms by Sector of Operation

	M&A Target Firms		
Sector	**Domestic**	**Cross-Border**	**Total (%)**
Agriculture	25	19	1.90%
Forestry	4	0	0.17%
Oil and Natural Gas Extraction	1	6	0.30%
Metallic Mineral Mining	7	6	0.56%
Non-Metallic Mineral Mining	5	2	0.30%
Food and Beverage Manufacturing	108	62	7.35%
Tobacco Products Manufacturing	1	1	0.09%
Textile Products Manufacturing	12	3	0.65%
Apparel Manufacturing	6	4	0.43%
Leather Processing, Luggage and Footwear Manufacturing	5	4	0.39%
Wood Products Manufacturing	2	2	0.17%
Pulp, Paper and Paper Products Manufacturing	22	20	1.82%
Publishing, Printing and Reproduction of Recordings	24	16	1.73%
Coal and Nuclear Products, Oil Refining and Alcohol Production	12	7	0.82%
Chemical Products Manufacturing	67	75	6.14%
Rubber and Plastics Product Manufacturing	15	30	1.94%
Nonmetallic Mineral Product Manufacturing	23	10	1.43%
Metals Production and Basic Processing	9	12	0.91%
Metal Product Manufacturing	10	28	1.64%
Machinery and Equipment Manufacturing	24	43	2.90%
Office and Data Processing Equipment Manufacturing	2	6	0.35%
Electrical Machinery and Equipment Manufacturing	6	18	1.04%
Electronic Components Manufacturing	7	10	0.73%
Medical Equipment, Optical and Precision Instruments	4	12	0.69%
Motor Vehicle Assembly and Manufacturing	7	13	0.86%
Other Transportation Equipment Manufacturing	3	6	0.39%
Furniture and Miscellaneome Manufacturing	4	2	0.26%
Recycling	1	1	0.09%
Production and Distribution of Energy	27	17	1.90%
Treatment and Distribution of Water	5	0	0.22%
Construction Services	27	18	1.94%
Automotives and Fuels Trade	14	9	0.99%
Wholesale Trade	74	87	6.96%
Retail Trade and Repairs	82	44	5.45%
Hospitality and Food	12	13	1.08%
Ground Transportation	26	6	1.38%
Maritime Transportation	1	2	0.13%
Aviation	9	5	0.61%
Auxiliary Transportation	70	18	3.80%
Telecommunications	34	10	1.90%
Financial Services	35	10	1.94%
Insurance	29	17	1.99%
Auxiliary Financial Services	19	20	1.69%
Real Estate Services	52	6	2.51%
Rentals	8	3	0.48%
IT and Software Related Activities	100	71	7.39%
R&D	2	2	0.17%
Advertising, Auditing, Consulting and Other Corporate Services	128	152	12.10%
Public Administration, Defense, and Social Security	2	1	0.13%
Education	90	11	4.36%
Health and Social Services	70	1	3.07%
Sewage and Cleaning Services	6	1	0.30%
Associative Activities	3	6	0.39%
Recreational, Cultural and Sports Activities	16	5	0.91%
Laundry, Dry Cleaning and Fitness Activities	2	2	0.17%

Notes: Sector of Operation (2-Digit CNAE) at the Time of the M&A Transaction.

Table 1.3: Summary Statistics - Treated Vs. Control Firms

Panel A: Firm Characteristics - Pre-Period							
	Treated Firms			Control Firms			
Variables	**p50**	**Mean**	**Std Dev.**	**p50**	**Mean**	**Std Dev.**	**Difference**
Number of Employees	118	613	1,691	123	568	1,285	45
Total Wage Bill (R$)	224,070	1,216,405	4,570,886	187,602	1,156,268	3,907,803	36,468
Log Employment	4.8	4.8	1.8	4.9	4.8	1.7	0
Log Total Wage Bill	12	12	2	12	12	1.8	0
Routine Share	0.80	0.72	0.24	0.78	0.72	0.18	0
High-Skilled Share	0.16	0.26	0.26	0.14	0.23	0.21	0.03
Number of Firms	2,204			20,257			

Panel B: Firm Characteristics (t = 0)							
	Treated Firms			Control Firms			
Variables	**p50**	**Mean**	**Std Dev.**	**p50**	**Mean**	**Std Dev.**	**Difference**
Number of Employees	139	678	1,672	132	581	1,305	97
Total Wage Bill (R$)	231,753	1,301,086	4,135,980	215,063	1,063,845	3,791,463	237,241
Log Employment	4.9	4.9	1.7	4.9	4.9	1.6	0
Log Total Wage Bill	12	12	1.8	12	12	1.8	0
Routine Share	0.79	0.72	0.24	0.80	0.74	0.18	-0.02
High-Skilled Share	0.16	0.27	0.26	0.14	0.24	0.22	0.03
Number of Firms	2,204			20,257			

Panel C: Workers' Characteristics (t = 0)							
	Treated Employees			Control Employees			
Variables	**p50**	**Mean**	**Std Dev.**	**p50**	**Mean**	**Std Dev.**	**Difference**
Education	7	6.4	1.9	7	6.1	1.9	0.3
Male	1	0.65	0.52	1	0.63	0.48	0.2
Age	30	32	9.1	30	32	9.3	0
Tenure (in Months)	14	37	57	13	32	48	5
Log(Wage)	6.9	7	1.3	6.8	6.9	1.1	0.1
Number of Workers	2,281,039			9,021,397			

Notes: The table reports descriptive statistics: **(I)** at the firm-level for the pre-takeover period (Panel A), **(II)** at the firm-level at the time of the M&A transaction (Panel B) and, **(III)** at the worker-level at the time of the M&A transaction (Panel C). Education takes values from 1 to 11 ranging from Illiteracy to Doctoral Degree. An education level of 7 reflects completion of high school education.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.4: **Total Employment and Wages**

	(1)	(2)	(3)
Variables	**Log(L)**	**Log(Wages)**	**Log(Wages/L)**
Post	-0.036***	-0.105***	-0.030***
	(0.006)	(0.009)	(0.003)
Post $\times I_i^{Treated}$	-0.288***	-0.174***	0.143***
	(0.019)	(0.031)	(0.008)
Firm Fixed Effects	Yes	Yes	Yes
Municipality x Year Fixed Effects	Yes	Yes	Yes
Observations	148,060	148,060	148,060
Adjusted R^2	0.89	0.81	0.66

Notes: In Column (1) the dependent variable is firm-level employment. In Column (2) the dependent variable is firm-level log wages. In Column (3) the dependent variable is the firm-level log wage per employee. Employment is measured as the log number of employees in the firm. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.5: Labor Restructuring and Labor Flows

	(1)	(2)	(3)	(4)	(5)
Variables	**Separation Rate**	**Firing Rate**	**Voluntary Rate**	**Hiring Rate**	**Log(Occupations)**
Post	-0.039***	-0.030***	-0.009	-0.062***	-0.035***
	(0.003)	(0.002)	(0.007)	(0.007)	(0.003)
Post $\times I_i^{Treated}$	0.379***	0.326***	0.054***	-0.318**	-0.108***
	(0.038)	(0.034)	(0.008)	(0.156)	(0.011)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipality x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	146,874	146,874	146,874	146,874	148,060
Adjusted R^2	0.27	0.30	0.21	0.18	0.89

Notes: In Column (1) the dependent variable is the firm-level ratio of the number of separations at a specific year over employment in the previous year. In Column (2) the dependent variable is the firm-level ratio of the number of involuntary separations at a specific year over employment in the previous year. In Column (3) the dependent variable is the firm-level ratio of the number of voluntary separations at a specific year over employment in the previous year. In Column (4) the dependent variable is the firm-level ratio of the number of hirings at a specific year over employment in the previous year. In Column (5) the dependent variable is the log number of distinct occupational codes at a specific year. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.6: Labor Restructuring and Level of Skill

	High-Skilled Labor		Low-Skilled Labor	
	(1)	(2)	(3)	(4)
Variables	**Involuntary**	**Voluntary**	**Involuntary**	**Voluntary**
Post	0.050***	0.023***	0.020***	0.007***
	(0.003)	(0.002)	(0.005)	(0.001)
Post $\times I_j^{Treated}$	0.012	0.010***	0.059***	0.002
	(0.012)	(0.003)	(0.008)	(0.004)
Employee Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Observations	5,209,866	5,209,866	28,896,450	28,896,450
Adjusted R^2	0.32	0.30	0.35	0.39

Notes: In Columns (1) and (3) the dependent variable is an employee-level binary variable that equals 1 for involuntary separation at a specific year and 0 otherwise. In Columns (2) and (4) the dependent variable is an employee-level binary variable that equals 1 for voluntary separation at a specific year and 0 otherwise Columns (1) and (2) refer to the sample that includes only high-skilled labor, while Columns (3) and (4) refer to the sample that includes only low-skilled employees. High-skilled labor includes employees that have at least completed an undergraduate degree. Low-skilled labor includes employees that have at most received high-school education. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_j^{Treated}$ is an indicator function equal to 1 for individuals that have been employed in a firm that was the target of an M&A transaction at any point in time, and equal to 0 for the individuals of control firms. Employee controls include age, gender, tenure and education. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014. Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.7: Labor Restructuring and Managers

	Involuntary			Voluntary		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Variables	**Full Sample**	**Domestic**	**Cross-Border**	**Full Sample**	**Domestic**	**Cross-Border**
Post	0.042***	0.039***	0.048***	0.011***	0.011***	0.012***
	(0.004)	(0.006)	(0.007)	(0.001)	(0.002)	(0.002)
Post $\times I_j^{Treated}$	0.032***	0.040***	0.018	0.005	0.004	0.006
	(0.008)	(0.010)	(0.019)	(0.006)	(0.005)	(0.006)
Employee Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,685,446	882,692	802,663	1,685,446	882,692	802,663
Adjusted R^2	0.32	0.26	0.32	0.30	0.25	0.32

Notes: The sample includes employees in managerial positions. Managerial positions are occupations with a two-digit ISCO code equal to 12. In Columns (1)-(3) the dependent variable is an employee-level binary variable that equals 1 for involuntary separation at a specific year and 0 otherwise. In Columns (4)-(6) the dependent variable is an employee-level binary variable that equals 1 for voluntary separation at a specific year and 0 otherwise. Columns (1) and (4) examine the full sample of M&As. Columns (2) and (5) examine the sample of Domestic M&As, while Column (3) and (6) examine the sample of Cross-Border M&As. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_j^{Treated}$ is an indicator function equal to 1 for individuals that have been employed in a firm that was the target of an M&A transaction at any point in time, and equal to 0 for the individuals of control firms. Employee controls include age, gender, tenure and education. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.8: Labor Restructuring and Routine-Biased Change

	Involuntary Separation					
	Non-Routine			Routine		
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	**Full Sample**	**Domestic**	**Cross-Border**	**Full Sample**	**Domestic**	**Cross-Border**
Post	0.053***	0.042***	0.038***	0.014***	0.004	0.019**
	(0.006)	(0.008)	(0.009)	(0.011)	(0.007)	(0.008)
Post $\times I_j^{Treated}$	0.027**	0.037**	0.018	0.060***	0.071***	0.049***
	(0.012)	(0.017)	(0.021)	(0.007)	(0.010)	(0.008)
Employee Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,009,071	4,360,902	3,422,026	25,691,547	12,848,318	12,102,520
Adjusted R^2	0.38	0.34	0.38	0.34	0.27	0.36

Notes: The dependent variable is an employee-level binary variable that equals 1 for involuntary separation at a specific year and 0 otherwise. Columns (1)-(3) refer to the sample that includes only routine employees, while Columns (4)-(6) refer to the sample that includes only non-routine employees. Employees are categorized as Routine or Non-Routine based on their occupational Routine Task Intensity (RTI) Score. Routine employees have a positive RTI, while non-routine employees have a negative RTI. Columns (1) and (4) examine the full sample of M&As. Columns (2) and (5) examine the sample of Domestic M&As, while Column (3) and (6) examine the sample of Cross-Border M&As. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_j^{Treated}$ is an indicator function equal to 1 for individuals that have been employed in a firm that was the target of an M&A transaction at any point in time, and equal to 0 for the individuals of control firms. Employee controls include age, gender, tenure and education. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.9: Labor Restructuring - Hirings

	Skill Level		Routine Intensity	
	(1)	**(2)**	**(3)**	**(4)**
Variables	**Low-Skilled**	**High-Skilled**	**Routine**	**Non-Routine**
Post	-0.017***	0.009	-0.011*	0.001
	(0.006)	(0.010)	(0.006)	(0.011)
Post $\times I_j^{Treated}$	-0.055***	-0.009	-0.044***	-0.011
	(0.009)	(0.018)	(0.010)	(0.024)
Employee Controls	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Observations	28,896,486	5,209,967	25,691,547	8,009,071
Adjusted R^2	0.33	0.31	0.34	0.35

Notes: The table reports the effect of M&A on hirings. Columns (1) and (2) focus on high-skilled versus low-skilled employees, while Columns (3) and (4) focus on routine versus non-routine employees. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_j^{Treated}$ is an indicator function equal to 1 for employees that have been employed at a firm targeted at an M&A transaction at any point in time, and equal to 0 for employees of control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.10: Human Capital Overlap

	Target Employees			Acquirer Employees		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Variables	**Voluntary**	**Involuntary**	**Involuntary**	**Voluntary**	**Involuntary**	**Involuntary**
Overlap	0.001	0.015***	0.018**	0.001	-0.001	-0.003
	(0.002)	(0.002)	(0.008)	(0.001)	(0.002)	(0.004)
Low-Skilled			0.080***			0.042***
			(0.020)			(0.006)
Low-Skilled $\times$ Overlap			0.010**			0.005
			(0.005)			(0.004)
Employee Controls	Yes	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,394,702	3,394,702	3,394,702	29,050,925	29,050,925	29,050,925
Adjusted R^2	0.45	0.42	0.44	0.48	0.41	0.42

Notes: The table reports the impact of human capital overlap on the likelihood of voluntary and involuntary separation in the post-takeover period. Columns (1)-(3) use the subsample of target employees in the post-takeover period, while (4)-(6) use the subsample of acquirer employees in the post-takeover period. In Columns (1) and (4) the dependent variable is an employee-level binary variable that equals 1 for voluntary separation at a specific year and 0 otherwise. In Columns (2), (3), (5) and (6) the dependent variable is an employee-level binary variable that equals 1 for involuntary separation at a specific year and 0 otherwise. Low-skilled labor includes employees that have at most received high-school education.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.11: Skill-Biased and Routine-Biased Change

	(1)	(2)	(3)
Variables	**High-Skilled Share**	**Routine Share**	**R&D Share**
Post	-0.002	0.003	0.001
	(0.002)	(0.002)	(0.001)
Post $\times I_i^{Treated}$	0.070***	-0.076***	0.012***
	(0.003)	(0.004)	(0.002)
Firm Fixed Effects	Yes	Yes	Yes
Municipality x Year Fixed Effects	Yes	Yes	Yes
Observations	148,060	148,060	148,060
Adjusted R^2	0.91	0.88	0.62

Notes: In Column (1) the dependent variable is the share of high-skilled labor at the firm level. In Column (2) the dependent variable is the firm-level share of routine labor. In Column (3) the dependent variable is the firm-level share of labor in occupational tasks related to R&D. High-skilled labor includes employees that have at least completed an undergraduate degree. Routine labor includes employees that the Routine Task Intensity (RTI) Score is positive. The categorization in R&D occupations is based on the occupational code reported in RAIS. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014. Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.12: Labor Restructuring and Average Wages

	Average Wage	
	(1)	(2)
Variables	**High-Skilled Labor**	**Low-Skilled Labor**
Post	-0.007***	-0.034***
	(0.003)	(0.003)
Post $\times I_i^{Treated}$	0.049***	-0.146***
	(0.008)	(0.028)
Firm Fixed Effects	Yes	Yes
Municipality x Year Fixed Effects	Yes	Yes
Observations	148,060	148,060
Adjusted R^2	0.88	0.62

Notes: The dependent variable is average log wage at the firm-level. In Column (1) the dependent variable is measured including only the high-skilled labor of the firm. In Column (2) the dependent variable is measured including only the low-skilled labor of the firm. High-skilled labor includes employees that have at least completed an undergraduate degree. Low-skilled labor includes employees that have at most received high-school education. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014. Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.13: **Labor Restructuring and Wage Inequality**

	(1)	(2)
Variables	**Standard Deviation of Log Wages**	**90-10 Wage Ratio**
Post	-0.009	0.008
	(0.008)	(0.014)
Post $\times I_i^{Treated}$	0.068***	0.133***
	(0.012)	(0.026)
Firm Fixed Effects	Yes	Yes
Municipality x Year Fixed Effects	Yes	Yes
Observations	148,060	148,060
Adjusted R^2	0.60	0.68

Notes: The dependent variable is a measure of wage inequality. In Column (1) the relevant measure is the standard deviation of log wages at the firm level, while in Column (2) the firm-level ratio of the 90th wage percentile to the 10th wage percentile (Column (2)). *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.14: Combined Entity

	(1)	(2)	(3)	(4)	(5)
Variables	**Log(L)**	**Log(Wages)**	**High-Skilled Share**	**Routine Share**	**90-10 Wage Ratio**
Post	-0.080***	-0.113***	-0.002	0.003	0.004
	(0.012)	(0.014)	(0.002)	(0.002)	(0.012)
Post $\times I_i^{Treated}$	-0.129***	-0.099***	0.043***	-0.048***	0.096***
	(0.025)	(0.029)	(0.002)	(0.002)	(0.023)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipality x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	219,469	219,469	219,469	219,469	219,469
Adjusted R^2	0.88	0.80	0.90	0.85	0.71

Notes: The dependent variable is one of the following: log employment (Column (1)), log wages (Column (2)), share of high-skilled employees (Column (3)), share of routine employees (Column (4)), and wage inequality (Column (5)). *Post* is a dummy that equals 1 for the two-year period after the M&A transaction was withdrawn, and 0 for the the two-year period prior to the withdrawal of the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time that has been subsequently withdrawn, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.15: **Labor Restructuring - Takeover Types**

	Combined Entity			
	Log(L)		Log(Wages)	
	(1)	**(2)**	**(3)**	**(4)**
Variables	**Focused M&As**	**Diversifying M&As**	**Focused M&As**	**Diversifying M&As**
Post	-0.087***	-0.051***	-0.116***	-0.092***
	(0.014)	(0.011)	(0.015)	(0.016)
Post $\times I_i^{Treated}$	-0.153***	0.025	-0.117***	0.039
	(0.028)	(0.048)	(0.038)	(0.041)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Municipality x Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	179,908	39,517	179,908	39,517
Adjusted R^2	0.88	0.91	0.80	0.84

Notes: The table reports the effect of different types of M&As on total employment and total wage bill of the combined entity. In Columns (1) and (2) the dependent variable is the log number of employees, while in Columns (3) and (4) the dependent variable is the log total wage bill. Columns (1) and (3) present results for focused M&As and Columns (2) and (4) present results for diversifying M&As. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.16: Labor Restructuring - Human Capital Relatedness

	Combined Entity			
	Log(L)		Log(Wages)	
	(1)	**(2)**	**(3)**	**(4)**
Variables	**High HCR M&As**	**Low HCR M&As**	**High HCR M&As**	**Low HCR M&As**
Post	-0.024**	-0.081***	-0.107***	-0.090***
	(0.012)	(0.017)	(0.024)	(0.016)
Post $\times I_i^{Treated}$	-0.193***	-0.095**	-0.164***	-0.041***
	(0.036)	(0.046)	(0.048)	(0.006)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Municipality x Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	92,247	83,513	92,247	83,513
Adjusted R^2	0.89	0.89	0.81	0.81

Notes: The table reports the effect of M&As with different level of Human Capital Relatedness on total employment and total wage bill of the combined entity. In Columns (1) and (2) the dependent variable is the log number of employees, while in Columns (3) and (4) the dependent variable is the log total wage bill. Columns (1) and (3) present results for M&As with a high level of human capital relatedness and Columns (2) and (4) present results for M&As with a low level of human capital relatedness. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.17: Withdrawn M&As

	(1)	(2)
Variables	**Log(Employment)**	**Log(Wages)**
Post	-0.018	-0.076
	(0.029)	(0.040)
Post $\times I_i^{Treated}$	-0.007	0.063
	(0.020)	(0.107)
Firm Fixed Effects	Yes	Yes
Municipality x Year Fixed Effects	Yes	Yes
Observations	51,519	51,519
Adjusted R^2	0.93	0.86

Notes: The dependent variable is either log employment or log wages. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction was withdrawn, and 0 for the the two-year period prior to the withdrawal of the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time that has been subsequently withdrawn, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.18: Investment in Capital

	Panel A: Summary Statistics at t = 0						
	Treated Firms			Control Firms			
Variables	**Mean**	**p50**	**Std Dev.**	**Mean**	**p50**	**Std Dev.**	**Difference**
Log(Assets)	7.46	7.74 7.46	1.66	7.36	7.72	1.85	0.10
Leverage	0.33	0.29	0.42	0.32	0.28	0.50	0.01
Cash	0.12	0.10	0.11	0.11	0.09	0.12	0.01
CapEx	0.08	0.06	0.09	0.08	0.05	0.11	0
PPE Growth	0.05	-0.02	0.16	0.06	-0.01	0.24	-0.01
Intangible Assets Growth	0.06	0.00	0.16	0.04	-0.01	42.56	0.02
Number of Firms	82			421			

	Panel B: Regression Analysis		
	(1)	(2)	(3)
Variables	**Intangible Assets Growth**	**PPE Growth**	**CapEx**
Post	-5.15	-4.40	-0.01
	(10.09)	(3.39)	(0.01)
Post $\times I_i^{Treated}$	55.02**	3.46**	0.027**
	(26.73)	(1.74)	(0.13)
Firm Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes
Observations	2,151	3,156	2,111
Adjusted R^2	0.34	0.43	0.34

Notes: Panel A reports descriptive statistics of firm-level financial variables for public firms included in Compustat. The variables included are the natural logarithm of total assets (Compustat Item AT), leverage ((Compustat Item DLC + Compustat Item DLTT)/Compustat Item AT), cash (Compustat Item CHE)/Compustat Item AT), CapEx (Compustat Item CAPX)/Compustat Item AT(t-1)) and PPE growth (Compustat Item PPENT/Compustat Item PPENT(t-1) -1). Panel B reports the results of a univariate analysis that compares the change in capital expenditures between treated and control firms in the period before and after the takeover. Panel C report firm-level regression results for the sample of public firms. In Column (1) the dependent variable is Intangible Assets Growth, in Column (2) the dependent variable is PPE growth, while in Column (3) the dependent variable is CapEx. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_i^{Treated}$ is an indicator function equal to 1 for firms that have been targeted at an M&A transaction at any point in time, and equal to 0 for control firms that have never been involved in a M&A transaction either as targets or acquirers during the period under study. Firms controls include leverage, cash and sales growth (Compustat Item SALE).
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1.19: Labor Restructuring and Unemployment

	Labor Outcomes							
	Low-Skilled				High-Skilled			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	**Unemployed**	**Spell**	**Turnover**	**Log(Wage)**	**Unemployed**	**Spell**	**Turnover**	**Log(Wage)**
Post	0.041***	0.441***	0.079***	-0.059***	0.036***	0.366***	0.095***	-0.110***
	(0.004)	(0.041)	(0.008)	(0.009)	(0.003)	(0.029)	(0.007)	(0.007)
Post $\times I_j^{Treated}$	0.033***	0.430***	0.053***	-0.056**	0.009	0.110	0.018	-0.021
	(0.007)	(0.078)	(0.016)	(0.024)	(0.008)	(0.056)	(0.013)	(0.019)
Employee Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,896,486	28,896,486	28,896,486	28,896,486	5,209,967	5,209,967	5,209,967	5,209,967
Adjusted R^2	0.37	0.42	0.47	0.50	0.35	0.37	0.37	0.48

Notes: The table reports results on employment outcomes of target employees in the post-takeover period. Columns (1)-(4) focus on low-skilled employees, while Columns (5)-(8) focus on high-skilled employees. The dependent variable is: **(I)** in Columns (1) and (5) an indicator variable that equals 1 for employees that have experienced displacement and unemployment in the 2-year period after their displacement and 0 otherwise, **(II)** in Columns (2) and (6) the sum of unemployment spells in months for employees that have experienced displacement and unemployment in the 2-year period after their displacement and 0 otherwise, **(III)** in Columns (3) and (7) the number of jobs for employees that have experienced displacement in the 2-year period after their displacement and 0 otherwise, and **(IV)** in Columns (4) and (8) the average monthly log wage during the year. High-skilled labor includes employees that have at least completed an undergraduate degree. Low-skilled labor includes employees that have at most received high-school education. *Post* is a dummy that equals 1 for the two-year period after the M&A transaction, and 0 for the the two-year period prior to the M&A transaction. $I_j^{Treated}$ is an indicator function equal to 1 for individuals that have been employed in a firm that was the target of an M&A transaction at any point in time, and equal to 0 for the individuals of control firms. Employee controls include age, gender, tenure and education. Standard errors are clustered at the transaction level. The sample period is from 2002 to 2014.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Chapter 2

Firm Growth and Labor Reallocation after Exposure of Corrupt Practices

2.1 Introduction

Recent studies using firm-level data have documented two stylized facts. First, the existence of large and persistent differences in size and productivity across firms even within narrowly-defined sectors. Second, that such differences are larger among firms that operate in developing countries than among firms that operate in developed countries (Hsieh and Klenow 2009). Several theoretical models have attributed this dispersion to frictions at the firm level, which prevent the reallocation of factors of production from low to high productivity firms. For example, some low-productivity firms might be kept artificially large by preferential access to finance, government subsidies, corporate tax policy or other regulation, at the expense of more productive firms whose growth is, in this way, hindered. Therefore, removing or attenuating these frictions should result in a lower dispersion in firm size and productivity, a better allocation of resources, and higher output (Restuccia and Rogerson 2008).

In this essay I study one potential source of such frictions: preferential assignment of procurement contracts by the government. In particular, I estimate the effect of disclosing corrupt practices of firms that have procurement contracts with local governments on firm growth and labor reallocation. To identify this effect, I exploit exogenous variation in the timing of exposure of illegally favored firms using random municipality audits by a large anti-corruption program in Brazil. In 2003, the Brazilian government started to randomly audit municipal budgets for their use of federal funds. Among the most common types of misuse of federal funds revealed by this auditing program are the absence of a proper tender process - which favored a specific company - or the over-invoicing for the provision of certain goods and services. Both companies and local politicians involved in these practices are reported. The audit reports are then published online and are available to the public and the popular press. From the audit reports, I manually collected information on the firms illegally favored by local government officials, as well as details on the nature of the misuse and the sums involved. When corrupt practices are revealed, consequences for firms involved vary from a ban to participate in future tender processes, restitution of misuse funds, pay of a penalty fee up to judicial action.

I match corrupt firms exposed in this program with detailed information on their labor force. I use the employer-employee dataset RAIS (Relação Anual de Informações Sociais) of the Brazilian Ministry of Labor (MTE), which records comprehensive employee-level information on wage, occupation, demographic characteristics along with employer tax identifier, location and sector of operation.[1] According to the Brazilian law, every private or public-

[1] To be more precise, I match the 14-digit firm fiscal identifier reported in the audit report with the fiscal identifier in the RAIS dataset. This code identifies a firm for single-plant firms, and a specific plant for

sector employer must report this information every year to the Ministry of Labor. The Ministry of Labor estimates that RAIS includes over 90 percent of formally employed individuals in Brazil.

I then use the random nature of the auditing program to estimate the effect of disclosure of corrupt practices on firm growth and labor reallocation. I find that firms exposed by the auditing program experience a decline in employment growth relative to similar firms operating in the same sector and municipality after public disclosure of the report. The magnitude of my estimates indicate that exposed firms experienced on average 1.1% lower monthly growth (13.2% annualized) in terms of employment relative to non-exposed firms in the year after the audit report. In addition, exposed firms are 5.4% more likely to exit from my sample within a year following disclosure of corrupt practices. There are two potential mechanisms that can explain this effect. Exposed firms might experience a negative shock to the present value of their future earnings coming from procurement contracts.[2] This could be because local politicians that used to favor them are less likely to be re-elected, as shown in Ferraz and Finan (2008, 2011). Or because, following exposure, the company might be banned from participating in future tender offers by local governments. Also, if the federal government is a customer itself of the same company, those contracts might also be lost. Another potential mechanism linking exposure to firm growth has to do with reputation. That is, exposed companies might face issues getting new credit, catering to new clients or hiring workers due to the exposure of their questionable ethical behavior.

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multi-plant firms. In the rest of the essay I will use the term firm to identify this unit of observation.

[2]The positive effect of procurement contracts on firm growth has been documented in Ferraz, Finan, and Szerman (2015).

Next, given the observed employment decline, I focus on exposed firms and investigate the heterogeneous effects on the probability of separation among employees with different characteristics. I observe that, after exposure, separation rates are higher for male, younger employees and employees with lower education. Furthermore, non-managerial employees are more likely to leave exposed firms.

Finally, and related to the previous point, I investigate the effect of this auditing program on labor reallocation. I find that municipality-sectors in which exposed firms operate experience a reduction in the dispersion of firm size after the disclosure of the audit report. This result is consistent with part of the workers released by exposed firms being hired by smaller firms operating in the same municipality-sector, and whose growth was previously limited by unfair competition. Crucially, I find that the share of workers that leave exposed firms and is then hired by another exposed firms substantially decreases after exposure. My results show that, conditional on finding another job, 17% of workers leaving exposed firms in the year *before* exposure end up working for another exposed firm. This share decreases to 9% in the year *after* exposure and to only 5% two years after exposure. In other words, these results suggest that exposure of corrupt practices generates a reallocation of workers from politically-connected firms to non-politically connected ones.

The essay adds to the literature investigating the effect of corruption on firm-level outcomes. There is a vast literature that shows that corruption reduces aggregate growth and has negative implications for welfare (e.g. Mauro 1995, Clague, Keefer, Knack, and Olson 1996, Hall and Jones 1999, La Porta and Lopez-de Silanes 1999, Shleifer and Vishny 1993, Glaeser and Saks 2006.)[3] Moreover prior studies have shown that preferential treatment of firm by politicians increase firm value (Fisman 2001, Johnson and Mitton 2003, Bortolotti and Faccio 2004 and Brogaard, Denes, and Duchin 2016). Furthermore I relate to the literature that studies the effects of revealing corruption in firms (Smith, Stettler, and Beedles 1984, Karpoff, Lee, and Martin 2013 and Zeume 2017).

My study both complements and adds to these literatures. By exploiting the random timing of the audits, I can precisely identify the effects of revealing corruption on firms and alleviate concerns that my results might be driven by aggregate trends. Furthermore, although it is not random which firms are caught, the random timing of the audits and the fact that I can observe all firms caught, overcomes reporting and timing concerns related to using prosecuted cases (i.e. many cases can be settled before going to prosecution stage) or cases related to voluntary disclosures. Moreover, my study offers new evidence in the literature on the effect of revealing corruption on labor reallocation. I show that exposing corrupt practices leads to increased separation rates in the firms, especially for young and less educated employees. Thus, my results highlight the role of preferential allocation for contracts as a potential friction generating labor misallocation. In this sense, my essay is also linked to the recent literature on the determinants of misallocation of capital and labor across firms, and their aggregate implications (Hsieh and Klenow 2009).

Several papers have exploited the Brazilian random audit reports experiment following the seminal work of Ferraz and Finan (2008, 2011). Most of the literature has used variation at municipality level to study the effect of exposure of corruption practices on electoral and

[3]Bardhan (1997) and Svensson (2005) offer a review of the literature on this topic.

economic outcomes. For example, Bologna and Ross (2015) use random audits of municipal governments to study the effect of corruption on the number of business establishment as reported in RAIS. My experiment is similar in nature but exploit information on specific firms whose corrupt practices have been exposed in the audit reports and study their performance around these episodes. In a contemporaneous study, Colonnelli and Prem (2017) examine the effect of the Brazilian anti-corruption program on local economic activity, as well as the direct effect of exposure of corrupt practices at firm-level employment, credit borrowing and investment. They show that, over the five years after exposure, exposed firms tend to perform better than comparable firms in terms of both financial and real outcomes. I differ from this study by focusing on the short run effects of the auditing program on employment growth, as well as by focusing on the establishments mentioned directly in the audit reports instead of the firms they belong to.

The rest of the essay proceeds as follows. In section 2.1.1, I describe the federal auditing program in more details. Then, in section 2.2.1, I describe my two main data sources: employer-employee information from the RAIS dataset and manually extracted information on firm corrupt practices from publicly available audit reports. In this section I also present a set of stylized facts and summary statistics on firms that were exposed by the anti-corruption program and how they compare with similar firms operating in the same sector and municipality. Section 2.2.3 presents a simple theoretical framework that I use to guide the empirical analysis. Finally, in Section 2.3 I present all my main empirical results.

2.1.1 Background: Brazil Anti-Corruption Program

The anticorruption program, which features a municipality-level random auditing, began in 2003. The audit process is conducted by the Controladoria-Geral da União (CGU). It audits the municipal governments' expenditures from federal funds. The program began by selecting 26 municipalities (one from each state in Brazil) per lottery, and later expanded to 60 municipalities per lottery. The random selection is performed monthly. The lottery draw event invites the press, political parties, and the civil society to join and spectate, for transparency purposes. The pool from which the lottery is drawn includes all municipalities with population less than 450,000; the pool includes approximately 92 percent of Brazil's 5,564 municipalities and 73 percent of Brazil's total population (Ferraz and Finan (2008)). The information is collected by auditors who travel to the municipality and manually reviews the governments expenditure documents. The auditors are hired competitively through an exam and earn a competitive salary. The audit process lasts about ten days. In addition to Brazil's federal accountability office (the Tribunal de Contas de União), public prosecutors, and the municipal legislative branch, the results of the audit are released on the internet and to the media. According to Ferraz and Finan (2008), the news of revealed corruption will likely reach the public through the local radio. From the mayors side, corruption commonly takes the form of frauds, usage of phantom firms, over-invoicing, and diverting resources. The firms involved in the corrupt behavior are discovered and reported along with the mayor.

There are several potential consequences for firms that are exposed by the auditing program. Firms can be barred from participating in future tendering processes for federal contracts. For example, Planam, an ambulance company, was found to overprice for the services provided. The company was subsequently declared illicit by CGU (Internal Audit Agency/Federal Comptroller) and barred from future public proposals. Furthermore exposed firms might have to pay penalties or return misused funds. In certain instances, firm owners might face judicial action.[4] Thus there are potentially severe repercussions for caught firms. My working assumption is that firms exposed by the reports are likely to face a negative shock to the present value of their future discounted cash flows.

2.2 Methods

2.2.1 Data

This essay uses two main data sources. First, I use employer-employee data covering all formal workers operating in Brazil from RAIS (Relação Anual de Informações Sociais). Second, I use manually extracted information on firm identifiers and type of misconduct from the audit report published online by the Controladoria-Geral da União (CGU).

2.2.1.1 RAIS Data

Information on employer-employee relationship contained in RAIS data is collected by the Ministry of Labor (MTE). Although the data is collected annually, it can be retrospectively analyzed at a monthly frequency as each observation reports the starting and ending month of employer-employee relationship. RAIS is published at the employee-level, and the government requires it to cover employees from all formal workers (private and public sector). The MTE estimates that RAIS includes over 90 percent of formally employed individuals in

[4] According to the audit reports: "Irregular practices are forwarded to the Public Ministry and the TCU (Federal Court of Accounts) for penal action, the CGU for civil action of returning misused funds, and to Congress."

Brazil. The RAIS data include information regarding starting/ending dates of employment, occupation type, wage level, and demographic characteristics. The ending date is available in the data if the employee was separated from the job in that year. Interpreting layoffs may be complicated by mock firing then rehiring for receiving severance pay, a common practice found in Brazil. Available demographic characteristics include gender, age, and education level. The occupation type is coded according to the Classificação Brasileira de Ocupações (CBO). Two versions of the CBO code are reported (one version from 1994 and another from 2002); the CBO codes from the 1994 publication are mapped to the standard international occupation classification (ISCO). This concordance follows the approach developed by Muendler, Poole, Ramey, and Wajnberg (2004). I restrict my sample to workers aged 15 to 55. This is to avoid early retirement affecting entry and exit of workers in the data.[5]

2.2.1.2 Data from Auditing Reports

Information on the identity of corrupt firms is sourced from the audit reports published online by the Controladoria Geral da União (CGU). I analyzed 2,144 audit reports. First I collected the names and social security numbers (CNPJ) of all firms mentioned in these reports. Second, I manually checked each firm to ensure that it was actually found to be connected with an irregularity. Finally, I extracted information on the identity of the corrupt firm, the type of misuse of federal funds in which the firm was involved, the Federal Ministry that was the original source of the funds, and the amount of money received by the firm. Using the procedure, I identified a total of 8,854 exposed firms.

[5] I use the same age restriction as Bustos, Caprettini, and Ponticelli (2016b).

Figure 2.1 reports the number of lotteries by year (solid black line) as well as the number of municipalities audited (dashed blue line) and the number of firms whose corrupt practices have been exposed by the program (dashed red line). The program started in 2003 and, for this essay, I focus on the 12 years of data between 2003 and 2014. As the figure shows, the number of lotteries – and of municipalities audited – has been decreasing over time, at least in part due to the decrease in resources allocated to this auditing program by the federal Government over time. It is interesting to notice how, despite the lower number of municipalities audited, the number of exposed firms has been relatively constant at least until 2010. This might be due to either federal officials getting better at detecting misuse of federal funds or to a secular increase in corrupt practices in the assignment of public procurement contracts. Notice also that, starting from 2011 when Brazil enters into a period of lower economic growth, the number of exposed firms decreases more than proportionally with respect to the number of audited municipalities per year.

Table 2.1 reports the main types of irregularities detected by the auditing program, as well as the average and median amount received by the exposed firms. As shown, the most frequent type of irregularity (38.24%) occurs at the tendering process stage. Standard examples include procurement contracts assigned without the tendering process ever being started, favoritism of a given firm in the tendering process and simulation of a competitive auction (e.g. two competitors with the same address). Other common irregularities include the existence of fake receipts - i.e. the company charges to local government expenses that were never incurred - and different types of violation of the contract terms - a common one being late delivery of goods and services. Notice that if I focus on firms with large procurement contracts, the most frequent irregularity is overpricing, collusion with competitors and other irregularities in the tendering process.

Tables 2.2 and 2.3 report, respectively, Ministries and Federal Spending Programs that are the predominant sources of federal funds and where some type of misuse is detected at the local level. It is important to remember here that the source of funding is the federal government but the funds are allocated by municipality-level governments. As shown, the majority of misused funds originates from the Health and Education Ministries. Detected misuses include the overpricing of medicines for hospitals or the overpricing of school meals and desks, up to the improper acquisition of a school bus then used for the political campaign of the local major.

2.2.2 Descriptive Statistics on Exposed vs Non-Exposed Firms

In this section I present a set of descriptive statistics and basic stylized facts on the firms whose corrupt practices have been exposed by the auditing program. As I mentioned in section 2.2.1.2, I manually analyzed 2,144 audit reports from 39 lotteries between the year 2003 to the year 2014, and found a total of 8,854 exposed firms. Figure 2.2 shows the number of exposed firms (in units) by lottery. As reported in section 2.1.1, the number of municipalities extracted per lottery has changed over time, which in part explains the variation in the number of exposed firms. Figure 2.2 additionally separates the number of exposed firms by size category. I use the firm size categories used by the Brazilian National Statistical Institute (IBGE), which are based on number of employees. The IBGE defines as *Micro*, firms that have between 1 and 9 employees, as *Small*, firms that employ between 10 and 49 employees, as *Medium*, firms that have between 50 and 99 employees, and as *Large*, firms with 100 or more employees.[6]

Table 2.4 compares the share of exposed firms in each size category at the time of the lottery with the correspondent share in the population of non-exposed firms present in my data (around 7.1 million). As shown, exposed firms are on average larger than non-exposed firms. In particular, 36.1 % of exposed firms have at least 10 employees compared to 11.4% in the total population of firms.

Table 2.5 presents both firm-level and worker-level summary statistics for exposed and non-exposed firms, as observed at the time of the lottery. In Panel A, I show summary statistics at the firm level based on 8,854 unique exposed firms and 7,124,669 non-exposed firms.[7] As shown, exposed firms are around 4 to 5 times larger in terms of number of employees at the time of the lottery. The average size of exposed firms is 53 employees against the 12 employees of the non-exposed ones. This difference is even larger in terms of total wage bill. This is consistent with the idea that firms successfully catering to the demand for goods and services by local governments can rely on relative steady demand as well as large orders, and therefore grow more. In addition, exposed firms pay, on average, 2% larger salary per worker.

[6] I add an additional residual category, which I label "Zero" and which captures exposed firms that have zero employees at the time the lottery took place. As explained in section 2.2.1, the RAIS database only covers formal firms that have at least 1 employee. However, notice that I build my dataset at firm-month level starting from contract-level information. This means that a firm that has one employee for at least one month in a given year will show up in my data with one employee in that month and then with zero employees in all other months. I will disregard the "Zero" firms in the empirical analysis.

[7] The number of observations is higher since several exposed firms have been "exposed" more than once. Each exposed firm is then compared with all non-exposed firms operating in the same industry, municipality, and size category at the time of exposure. This means that I construct a different group of non-exposed peer firms for each exposed firm, and several non-exposed firm enter in several of these groups (which explains the large number of N). Similar logic applies to Panel B when I look at workers' characteristics.

In Panel B, I report summary statistics at the worker level, which are based on 444,065 unique workers of exposed firms and 84,747,871 unique workers of non-exposed firms. As shown, workers of exposed firms display, on average: slightly higher levels of education, lower share of female employees, higher average age and higher average tenure in the firm with respect to workers in non-exposed firms.

Table 2.6 shows the distribution of exposed and non-exposed firms by 2-digit sector. The distribution of exposed firms is skewed towards the services sector, and in particular retail trade and construction. These two sectors alone account for more than 50% of the exposed firms. Both agriculture and manufacturing have a lower share of exposed firms with respect to non-exposed firms.

Finally, I look at the geographical location of exposed firms. Notice that firms that are exposed by the auditing program are not necessarily located in the same municipality extracted in the lottery. This is because local governments are not required to purchase goods and services from local firms. Figure 2.3 shows the number and the share of exposed firms located in the audited municipalities by lottery. Around 70% of exposed firms were operating outside the municipality that was audited at the time of the lottery.

2.2.3 Theoretical Framework

In this section I present a simple theoretical framework to guide the empirical analysis. In this framework, firms use labor to produce a final good. One example is a catering firm producing meals for hospitals.[8] Firms are price takers and the market price for the final

[8]As showed in Table 2.6, the vast majority of exposed firms in my sample operate in the services and construction sectors.

good – in my example, an hospital meal – is P. I assume that firms are heterogeneous in two dimensions: their initial productivity and their connection to the local government. This connection allows certain firms to be more likely to win a procurement contract. In my example, connected firms are more likely to win a contract to cater a large public hospital. Therefore, these firms receive a boost in revenues from their connections, which is captured by τ. I can write the value of output of firm i as:

$$PY_i = \begin{cases} (1+\tau)P(A_i L_i^{\alpha}) & \text{if } i \text{ is government-favored} \\ \\ P(A_i L_i^{\alpha}) & \text{if } i \text{ is not government-favored} \end{cases} \tag{2.1}$$

where A_i and L_i are, respectively, physical productivity and number of workers of firm i. Profits for firm i can be written as:

$$\pi_i = \begin{cases} (1+\tau)PY_i - wL_i & \text{if } i \text{ is government-favored} \\ \\ PY_i - wL_i & \text{if } i \text{ is not government-favored} \end{cases} \tag{2.2}$$

Standard FOC for labor implies that the value of marginal product of labor is equal to the equilibrium wage:

$$PMPL_i = w = \begin{cases} (1+\tau)PMPL_i = (1+\tau)P\alpha\frac{Y_i}{L_i} & \text{if } i \text{ is government-favored} \\ \\ PMPL_i = P\alpha\frac{Y_i}{L_i} & \text{if } i \text{ is not government-favored} \end{cases} \tag{2.3}$$

Consider now two firms that operate in the same sector – the catering business. Suppose now that firm 1 is a local-government-favored firm, while firm 2 is not. Assume also that $\tau > 0$. Then, given that the equilibrium wage w must equalize across firms, the following holds:

$$(1+\tau)PMPL_1 = w = PMPL_2$$

and:

$$MPL_1 = MPL_2/(1+\tau) \tag{2.4}$$

Since $\tau > 0$, equation 2.4 implies: $MPL_1 < MPL_2$. That is, in equilibrium government-favored firms have lower marginal productivity of labor. In the presence of frictions in the assignment of procurement contracts, firms connected with the local government are artificially larger. Labor is misallocated.

Let me now describe how I think about mapping this simple framework to the data. The basic idea is that exposing corrupt firms removes the wedge τ and let firms compete only on physical productivity.

In my simple theoretical framework, when $\tau = 0$, labor reallocates from government favored firms towards non government favored firms until marginal products are equalized. Therefore, I expect exposed firms to grow relatively less than non-exposed firms following the audit report. Notice that the timing with which τ goes from positive to zero is exogenous in the data, as the municipalities targeted by the auditing programs are randomly drawn. This is important in order to disentangle a shock to τ from other productivity shocks – for example, a change in A_i – which could also generate a reallocation of labor. In the data, I do not have a good proxy for firm physical productivity. Therefore, my experiment relies on the random timing of the exposure. My identification assumption is that the timing of a shock to τ is uncorrelated with other productivity shocks happening at the same time. Since my data is at monthly frequency, I believe this assumption is likely to hold.

Finally, notice that in my theoretical framework there is a unique final good and labor can freely reallocate across firms. In order to match these assumptions of the model with the data, when studying the effect of a decrease in τ on changes in employment I always compare firms operating in the same narrowly defined sector and in the same municipality.

2.3 Results

The objective of this section is to test the main predictions of the theoretical framework described in section 2.2.3. To this end, I proceed in two steps. First, I study the effect of exposure on firm growth and firm exit. My hypothesis is that, once corrupt practices are exposed by the auditing program, firms that used to receive favorable treatment in the assignment of procurement contracts by the local government no longer do. This translates into lower employment growth relative to comparable firms operating in the same sector and municipality, and, potentially, a higher probability of exit. Employees that are laid off by exposed firms as well as new entrants in the labor market should therefore be more likely to work for non-exposed firms, at least until marginal products of labor across firms are equalized. In a second step, I study heterogeneous effects of the auditing program across workers of different types. Here I am particularly interested in whether exposed firms tend to lay off relatively more skilled versus unskilled employees, their probability of finding a new job, as well as which type of job they find in terms of sector, location, and occupation.

2.3.1 Main Effects of Exposure on Firm Growth

The objective of this section is to document the main effect of being exposed by the auditing program on firm growth. To this end, I estimate the following difference-in-differences specification:

$$\Delta \log y_{imjt} = \alpha_i + \alpha_{mt} + \alpha_{jt} + \sum_{p=-12}^{p=+12} \gamma_p(Month_p) + \sum_{p=-12}^{p=+12} \beta_p(I_i^{Exposed} \times Month_p) + \varepsilon_{imjt} \quad (2.5)$$

where i indexes firms, t indexes time – which is expressed in months – m indexes municipalities and j indexes sectors. I would like to compare exposed firms with non-exposed firms that are as similar as possible. To this end, I add to my specification a set of municipality and sector fixed effects interacted with monthly fixed effects (α_{mt} and α_{jt}). Sectors are defined according to the 2-digit CNAE Brazilian classification, which distinguishes between the 59 sectors reported in Table 2.6 and described in section 2.2.1. The variable $I_i^{Exposed}$ is an indicator function equal to 1 for firms that have been exposed by the auditing program at any point in time, and equal to 0 for firms that are never exposed during the period under study.[9] Finally, $Month_p$ is a dummy equal to 1 if $Month_p = p$. The coefficient of interest is β_p, which captures the average difference in firm growth between exposed and non-exposed firms in the same sector and located in the same municipality when $Month_p = p$.[10] Notice that p is expressed relative to the exposure month, which I denote as $p = 0$. This means that $p = 1$ indicates the first month after a firm has been exposed, and β_1 captures the average difference in firm growth between exposed and non-exposed firms one month after exposure.

[9] This implies that firms that are exposed by audit reports at any point in time are never used as "control" group in periods outside the two years around their exposure.

[10] I assign firms to municipality and sector as reported in my data at time $p = 0$.

My working hypothesis is that exposed firms plausibly receive a negative shock to public procurement contracts $after$ the audit report.

I construct the control group in three alternative ways. In the first approach, for each lottery I take a 10% random sample of non-exposed firms to be used as control. The random sample is created using firms that are operating at $p = 0$, the month of the lottery. In the second approach, for each exposed firm I use as control group all the firms in the same municipality and sector that exist at $p = 0$, the month of the lottery. In the third approach, for each exposed firm I use as control group all the firms in the same municipality, sector and size category that exist at $p = 0$, the month of the lottery. I use the fmy firm size categories used by the Brazilian National Statistical Institute, as I describe them in Section 2.2.2. In all three approaches, I then normalize time for both the treated and control group and follow firms before and after the audit. I am going to focus on exposed and non-exposed firms that are non-government agencies and located in municipalities that are never audited. The rationale is that by excluding firms located in audited municipalities we, at least in part, purge my estimates from any general equilibrium effects coming from changes triggered by the auditing program on the municipality as a whole. However, my results are unchanged if I focus on exposed firms regardless of geographical location.

Figure 2.4 reports the estimated β_p from equation 2.5 along with the 95 percent confidence interval, when the outcome variable is firm growth in terms of employment. Notice that in this specification I focus on a 24 months window around the month of the lottery. Also, the excluded month is $p = -12$. This implies that differences in firm growth between exposed and non-exposed firms are expressed relative to the same difference observed one year before the audit report.

As shown, exposed firms have a similar growth rate in terms of employment up to the month of exposure. Starting from the first month after the publication of the audit report, however, exposed firms have a relatively lower employment growth with respect to comparable firms operating in the same municipality and sector. Notice that the magnitude of the estimated coefficients suggests that exposed firms experience around 1 percentage point lower monthly growth after exposure. After around a year, the difference in firm growth tends to shrink. Notice that my finding that the effect of exposure on firm growth is temporary is consistent with my simple theoretical framework. As workers reallocated from exposed to non-exposed firms, the marginal product of labor of the latter decreases until it equalizes the one of exposed firms.

Next, I estimate the average effect of exposure on firm growth over the 12 months following the audit report as follows:

$$\Delta \log y_{imjt} = \alpha_i + \alpha_{mt} + \alpha_{jt} + \gamma Post_p + \beta(I_i^{Exposed} \times Post_p) + \varepsilon_{imjt} \tag{2.6}$$

where the variable $Post_p$ is a dummy equal to 1 for the twelve months following the audit report, and zero for the twelve months before it. Differently from equation 2.5, where I estimate the effect of exposure separately for each month, in equation 2.6 I estimate a single β which captures the average difference in growth rates between exposed and non-exposed firms in the year after the audit report relative the year before the audit report. Panel A Column (1) of Table 2.7 reports the results using as control group a 10% random sample of non-exposed firms. The magnitude of the estimated coefficient indicates that exposed firms experienced on average 1.1% lower monthly growth (13.2% annualized) in terms of employment relative to non-exposed firms in the year after audit report. As I mentioned above I focus on exposed and non-exposed firms that are located in municipalities that are never audited, thus my results are not likely to be driven by general equilibrium effects. In Column (2), I use an alternative control group, composed by non-exposed firms that operate in the same sector and municipality. In Column (3), in addition to municipality and sector, I match on size category. Matching on location, sector and size category reduces my point estimates by half. As shown, the estimated coefficient reported in Column (3) indicates that exposed firms experienced on average 0.4% lower monthly employment growth relative to comparable firms. In unreported regressions, I find similar results if I use wage bill as an alternative outcome.

Furthermore, in Panel B Table 2.7 I estimate the same equation extending the post-exposure period to 24 months. The results show that the effect still holds in the two-year horizon, and the estimated coefficients are of similar size as those in Panel A.

2.3.2 Main Effects of Exposure on Exit

In this section I study the effect of exposure of corrupt practices on firm probability of exit. To this end, I run a simple linear probability model similar to equation (2.6) in the previous section, as follows:

$$Pr(Exit_{imjt} = 1) = \alpha_i + \alpha_{mt} + \alpha_{jt} + \beta_1 Post_p + \beta_2 (I_i^{Exposed} \times Post_p) + \varepsilon_{imjt} \qquad (2.7)$$

where $Pr(Exit_{imjt} = 1)$ is a dummy equal to 1 if firm i exits from my sample in month t, while the variables $I_i^{Exposed}$ and $Post_p$ are defined as in equation (2.6). I define $Exit = 1$ if a firm exits the RAIS sample and it is never observed entering again during the period under study.[11] The coefficient of interest is β, which captures the difference in monthly probability of exit between exposed firms and comparable non-exposed firm before and after exposure.

The results of estimating equation 2.7 are reported in Table 2.8. As shown, exposed firms have a statistically significant higher probability of exit with respect to comparable non-exposed firms in the period under exposure. Panel A Column (1) of Table 2.8 reports the results using as control group a 10% random sample of non-exposed firms. The magnitude of the estimated coefficient indicates that exposed firms experienced on average 0.45% lower monthly growth in terms of employment relative to non-exposed firms in the year after audit report. Given that the mean monthly probability of exit in the sample is 0.4%, the result implies that the probability of exit doubles after exposure. Over the twelve months after exposure this translates into 5.4% higher probability of exit, or 478 firms out of the 8,854 which are exposed during the period under study. Moreover, in Column (2) I use an alternative control group, where I match by sector and municipality and in Column (3), in addition to municipality and sector I match on size category. Again, matching on location, sector and size category reduces the magnitude of my point estimates, which, however, remains positive and statistically significant.

Furthermore, in Panel B Table 2.8 I investigate the effect on exit over a longer horizon. I follow firms 24 months after exposure and I find similar results.

[11] In what follows I will interpret exit as going "out of business". However, notice that I can not disentangle a firm that goes out of business from a firm that loses all its employees but it is still operating as a business run by a self-employed owner, or a firm that becomes informal and stops reporting to the Ministry of Labor.

2.3.3 Labor Reallocation

In this section I study in more detail the effect of exposure of corrupt practices on labor reallocation. I start with a municipality-level analysis. My simple theoretical framework predicts that a decrease in τ, the price wedge favoring corrupt firms, should translate into a reallocation of workers from low to high productivity firms until marginal products of labor are equalized. As I do not observe MPL at firm level, in section 2.3.3.1 I use firm size as a proxy for MPL and study the effect of the auditing process on the distribution of firm size at municipality level. In particular, I test whether audited municipalities experience a decrease in dispersion of firm size relative to non-audited ones. Next, in section 2.3.3.2, I exploit the detailed nature of my data and follow workers that are laid off by exposed firms. My objective is to trace the flow of released workers following firm exposure and study the characteristics of the destination firms they end up working for in terms of size, industry and location. I also exploit the contract-level nature of my data to look at how exposure of corrupt practices affect unemployment spells.

2.3.3.1 Municipality-level Analysis: Firm Size Dispersion

I start by studying the effect of auditing on the dispersion of firm size at municipality level. My experiment aims at capturing the effect of removing favoritism in the allocation of procurement contracts on the firm size distribution. My simple theoretical framework predicts that a decrease in favoritism – i.e. a reduction in τ – should translate into a reallocation of workers from low to high productivity firms and a convergence in marginal products of labor within an industry. As my data do not report information of firm value added, I do not observe firm average labor productivity and cannot directly test this prediction. However, as an imperfect proxy for labor misallocation, I can test whether the auditing program had an effect on the dispersion of the within-sector firm size distribution in a given municipality. To this end, I estimate the following specification at municipality-month level:

$$st.dev.\left[\log\left(\frac{L_{ijt}}{\overline{L_{jt}}}\right)\right]_{mt} = \alpha_m + \alpha_t + \beta_1 Post_p + \beta_2 Audited_m \times Post_p + \varepsilon_{mt} \tag{2.8}$$

where $Audited_m$ is a dummy equal to 1 if a municipality has been audited and $Post_p = 1$ is a dummy equal to 1 for the 24 months after the municipality is extracted for auditing ($p = 1$ to $p = 24$), and zero for the 24 months before. The outcome variable is my measure of firm-size dispersion in deviation from the industry mean ($\overline{L_{jt}}$) computed at national level. Table 2.9 reports the result of estimating equation (2.8). As shown, the coefficient on the interaction is negative and significant. The size of the estimated coefficient indicates that audited municipalities experienced a decrease in firm-size dispersion with respect to non-audited municipalities. The size of the estimated coefficient indicates that municipalities that are randomly audited experience a 2.3% of a standard deviation decrease in firm-size dispersion with respect to the control group. I interpret this finding as suggestive evidence that the auditing program might have decreased misallocation of labor across firms.[12]

2.3.3.2 Worker-level Analysis: Separations

In this section I study whether employees are more likely to leave firms after the revelation of corrupt practices. Furthermore I investigate the composition of employees who leave the

[12] In future version of this draft I aim at constructing a more precise estimate of labor productivity merging data from RAIS with data on firm output and value added from sector-specific surveys.

firm after the revelation of corruption. Specifically I investigate the characteristics of workers who leave the exposed firms.

Workers with different characteristics may have different preferences and incentives to leave the exposed companies. On one hand, more talented or high-level (managerial) employees might be more likely to leave because they have a larger set of available outside options. On the other hand, more talented workers or managerial employees are often considered to have more influence on company decisions, including the decision to embark in corrupt practices to win a procurement contract from the local government. In that sense, managers of exposed firms might suffer a higher reputation cost of being associated with such corrupt practices. This might create a stigma in the job market which limits their options outside the exposed firm.

Moreover, from the perspective of the firms, exposed firms are likely to lose the contracts as well as eligibility for future government contracts. As a response, on one hand they might decide to lay off younger and lower level employees, since it is less costly to fire them. On the other hand, they might decide to fire high level employees who are more likely to be responsible for the misconducts. The theoretical ambiguity that arises from the different economic forces makes it an interesting empirical question which type of employees are more likely to leave exposed firms.

To perform the analysis I estimate the following linear probability model:

$$Pr(Separation_{eijt} = 1 | I_i^{Exposed} = 1) = \alpha_e + \alpha_t + \alpha_j + \beta Post_p + u_{ijt} \tag{2.9}$$

where $Separation_{eijt}$ is a dummy variable that takes the value of one in the month the worker e leaves firm i and 0 otherwise, α_e is worker fixed effect, α_t is time fixed effect, and α_j is industry fixed effect.[13] The variable $Post_p$ is a dummy equal to 1 for the twelve months following the audit report, and zero for the twelve months before it. Notice that this analysis is performed within the sample of exposed firms, so conditional on $I_i^{Exposed} = 1$. The coefficient β measures the increase in the monthly probability of an employee leaving the firm after corrupt practices are revealed. All the regressions include employee fixed effects to account for time invariant-worker characteristics. I also include month fixed effects and industry fixed effects. I cluster standard errors at the firm level.

Table 2.10 reports the results of estimating equation (2.9). In Column (1) I examine separations in a window of 3 months before and 3 months after the exposure, while in Columns (2) and (3) I examine 6 and 12 months around the exposure, respectively. In Column (1) I find that employees of exposed firms have a 2.7% higher monthly probability to leave the firms in the 3 months after corruption is revealed, compared to the 3 months before corruption is revealed. Moreover, since the specifications include employee fixed effects, the coefficient shows that there is a within-employee increase in the probability of leaving the firm after corruption is revealed. Column (2) shows that when looking in a 6 month window, employees of exposed firms have 2.7% higher monthly probability to leave after the firm is exposed, while the coefficient becomes 2% when I look into a window of 12 months around the lottery.

Table 2.10 documents a within-worker increase in the probability of leaving exposed firms after corrupt practices are revealed. Next, I investigate what are the characteristics of

[13]The employee might be leaving the firm either voluntarily or because he is fired. In this analysis I do not distinguish the reason of the separation.

employees who leave exposed firms. I start by reporting average characteristics of workers of exposed firms in the month the firm is exposed by the auditing program, and compare them with average characteristics of separated workers. I define separated workers as those whose working relationship with the exposed firm is interrupted between month 0 and 12 after exposure.

The first column of Table 2.11 shows the characteristics of employees in exposed firms at the time of exposure. The second column, describes the characteristics of employees who depart the firms after corruption is exposed. I observe that while 59% of the workforce of exposed firms consists of men, male workers comprise 72% of the released employees. Thus male workers seem more likely to leave. In terms of education, workers with basic education (lower than high-school) comprise 37% of the workers of exposed firms but 47% of the leavers. On the other hand, employees with higher than high-school education are less likely to leave the exposed firms. Consistent with firing low-skilled, and lower-cost employees, I observe that younger employees are more likely to leave the firm. Although employees younger than 35 years old represent 49% of workers in exposed firms, they are 63% of the leavers. The vast majority of workers have indefinite contracts, so there is not much variation in the type of labor contracts. Finally, I classify the employees into managers and non-managerial employees. Consistently with my previous findings, I observe that although managers are 21% of the employees, they only comprise 17% of the released employees. To sum up, the summary statistics presented in Table 2.10 indicate that young, lower-educated and non-managerial employees are more likely to depart from the exposed firms. I next test this more formally.

To more formally investigate how different worker characteristics relate to the probability of a worker leaving the firm, I estimate equation 2.9 and interact the $Post_p$ dummy with a set of worker characteristics. Table 2.12 investigates whether male or female employees are more likely to leave exposed firms. Gender is a dummy variable that takes the value 1 for female employees and 0 for male employees. All the specifications include month fixed effects, employee fixed effects and industry fixed effects. I report the coefficient of the interaction *Post*Gender* (*Post* and *Gender* are subsumed by the fixed effects). Column (1) shows that looking at a window of 3 months before and after the audit, female employees have 1.3% lower monthly probability to leave the firms after corruption is revealed, compared to male employees. Relative to the average effect of 2.7% from Table 2.10, this estimate implies that female workers are 48% less likely to leave the exposed firms than the average employee. Columns (2) and (3) examine a window of 6 and 12 months around the audit and show similar results, although demonstrating a lower economic magnitude.

Table 2.13 investigates whether more educated employees are less or more likely to leave exposed firms. The education variable takes 3 distinct values. It takes the value 1 for employees with basic education, the value 2 for employees who have graduated high school and the value 3 for employees with higher than high-school education. All the specifications include month fixed effects, employee fixed effects and industry fixed effects. I report the coefficient of the interaction *Post* with dummies of *Education* (*Post* and *Education* are subsumed by the fixed effects). Column (1) shows results looking at a window of 3 months before and after the audit. The Basic education is the omitted category. The coefficient of the interaction $Post{*}Education(2-1)$ shows that employees with high school education are less likely to leave compared to employee with basic education. Also as the coefficient of the interaction $Post$*$Education(3-1)$ shows, employees with higher than high school education are less likely to leave exposed firms relative to employees with basic education. I find similar results for a window of 6 and 12 months around the audit. Overall, Table 2.13 shows that employees with low education are more likely to leave exposed firms after corruption is revealed.

In Table 2.14 I examine how age relates to the propensity to leave the exposed firms. I classify employees into four age groups. Age takes the value 1 for employees between 16-25 years old, the value 2 for 26-35 years old, the value 3 for 36-45 years old and the value 4 for 46-55 years old. All the specifications include month fixed effects, employee fixed effects and industry fixed effects. The 16-25 years old group is the omitted age group. Throughout Columns (1)-(3) I observe that the coefficients of the interaction of $Post$ with dummies of Age is negative, indicating that older employees are more likely to leave exposed firms after corruption is revealed relative to the youngest group.

Moreover, I examine whether managers are more likely to leave exposed firms. Table 2.15 presents the results. $Manager$ is an indicator variable that takes the value 1 for employees who hold a managerial position and 0 otherwise. In Column (1), which examines the 3-month window around the audit. The coefficient of the interaction $Post$*$Manager$ reveals that managers are 0.8% less likely to leave exposed firms, compared to non-managerial employees. Overall, the results shows that younger, less educated and non-managerial employees are more likely to leave exposed firms, after corruption is revealed. Thus the results are inconsistent with firms losing their most valuable workers.

Finally, I investigate labor reallocation across firms. In particular, I study the characteristics of firms that workers released from exposed firms end up working for. Table 2.16 reports the characteristics of destination firms in the period before and after exposure. I focus on the following dimensions of heterogeneity across destination firms: size, industry, location, and exposure. In Column (1) I focus on all workers employed by exposed firms at time $p = -12$, and look at their employment situation in month $p = 0$.[14] I use this as a counter factual of labor reallocation in ”normal times”, which is based on employees of the exposed firms who left the firms before corruption was revealed. In Column (2) I focus on all workers employed by exposed firms at time $p = 0$ – the time of exposure – and look at their employment situation in month $p = 12$, a year after exposure. Finally, in Column (3), I focus on all workers employed by exposed firms at time $p = 0$ – the time of exposure – and look at their employment situation in month $p = 24$, two years after exposure. As shown, labor reallocation follows similar patterns – in terms of direction of the flow – before and after exposure when I focus on size of destination firms. Around 65% of workers released from exposed firms are still unemployed at the end of the year, 21-22% of released employees move to firms of similar size category, while 9-10% move to smaller firms. Moreover, only 4% of released employees go to larger firms. These results are, at least in part, driven by the fact that exposed firms tend to be relatively large. Therefore, separated workers are more likely to find an occupation in either a smaller firm or a firm of the same size.

Results are relatively similar in the period before versus after exposure also when I look at the industry and the location of destination firms of released workers. Out of those moving

[14] Notice that this analysis does not take into account entrants between $p = -11$ and $p = 0$.

to other firms, in the pre-exposure year 46% move to a different industry. This percentage grows to 50% after 1 year from exposure, and to 54% after two years. Similarly, out of the workers moving to other firms, in the pre-exposure year 49% move to a different municipality. This percentage grows to 51% in the post-exposure period.

In the bottom panel of Table 2.16, I investigate whether workers leaving exposed firms are more likely to be hired by exposed or non-exposed firms. My results show that, on average, in the year before exposure, almost 1 out of 5 workers (17%) leaving exposed firms – and finding a new job – end up working for another exposed firm. This share substantially decreases in the first year after exposure, when only 1 out of 10 workers (9%) leaving exposed firms and finding a new job end up working for another exposed firm, while the remaining 91% is reallocated towards non-exposed firms. Notice that this share is even larger if one considers the two years after exposure. Column (3) shows that, out of all workers leaving exposed firms and working for another firm two years after exposure, only 5% work for an exposed firm, while 95% is employed by a non-exposed firm. These results are consistent with the main prediction of my theoretical framework which suggests that exposure of corrupt practices should generate a reallocation of workers from politically-connected and low marginal productivity of labor firms to non-politically connected but high marginal productivity of labor firms.

2.4 Conclusion

Corruption in the assignment of procurement contracts is largely diffused in both developed and developing countries. In this essay I exploit a unique experiment to estimate the effect of disclosing corrupt practices on firm-level and worker-level outcomes. I obtain variation in the disclosure of corrupt practices from random municipality audits by a large anti-corruption program introduced by the Brazilian government in 2003, which was aimed at monitoring local governments' use of federal funds. I manually collect information on the identity of all firms mentioned in the audit reports, and match them with detailed information on their labor force from social security data. I find that firms exposed by the auditing program experience a decline in employment growth and an increase in the probability of exit relative to their peers after public disclosure of the audit report. Given the observed employment decline, I investigate the heterogeneous effects on the probability of separation across employees with different characteristics. I document that young, less-educated workers that do not occupy a managerial position have higher probability to leave the exposed firms. Also, I find that the laid-off workers tend to reallocate into firms that are not found as being illegally favored during the period under study, which is consistent with a reduction in labor misallocation at local level.

2.5 Figures and Tables

2.5.1 Figures

Figure 2.1: Lotteries, Audited Municipalities and Exposed Firms over Time

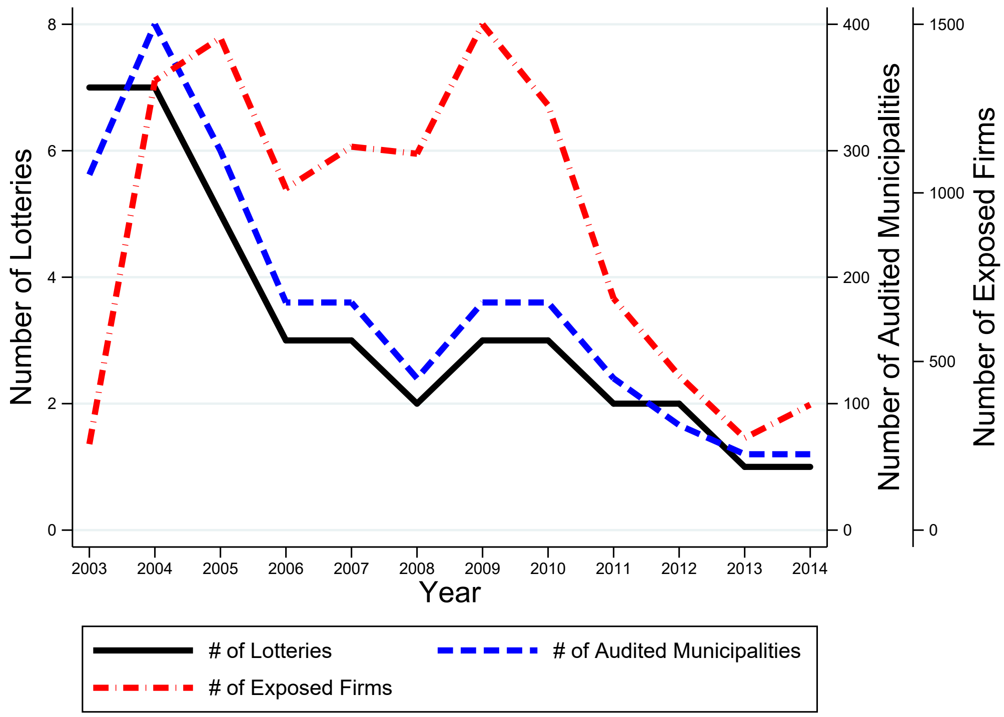


Figure 2.2: Exposed Firms by Size Category and Lottery Number

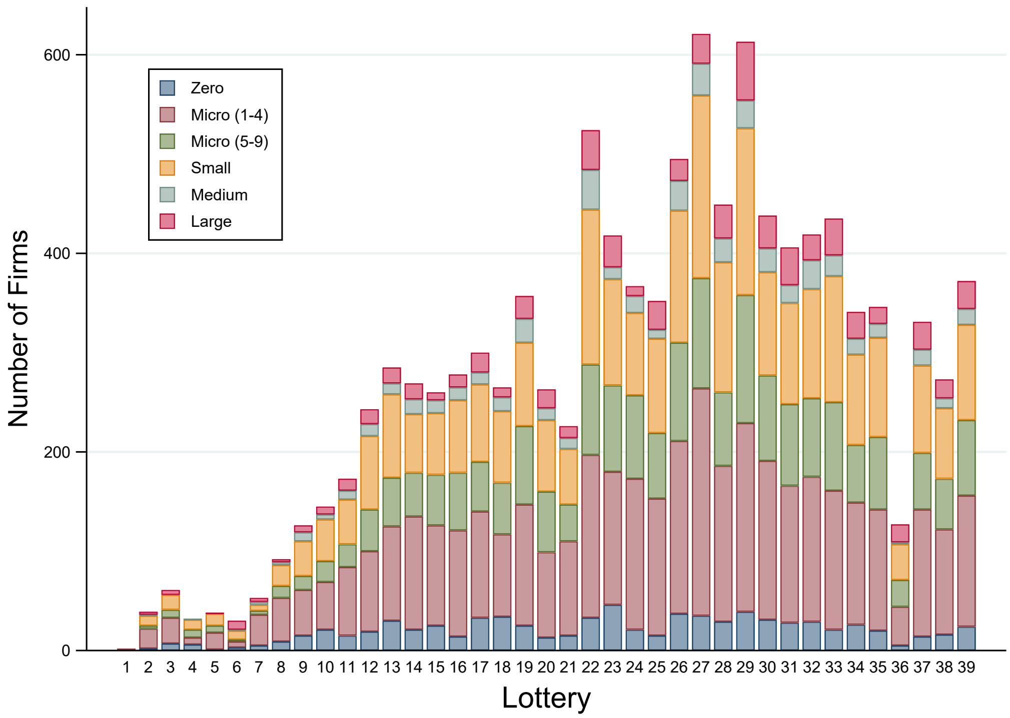


Figure 2.3: Exposed Firms by Location and Lottery

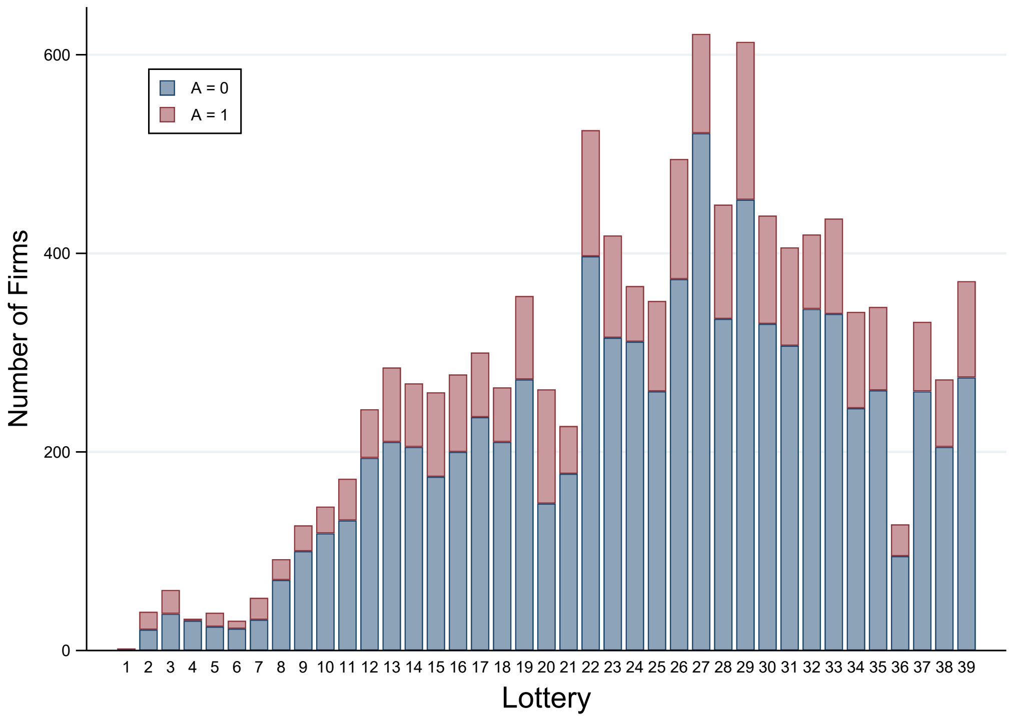


Figure 2.4: Effect of Exposure on Employment

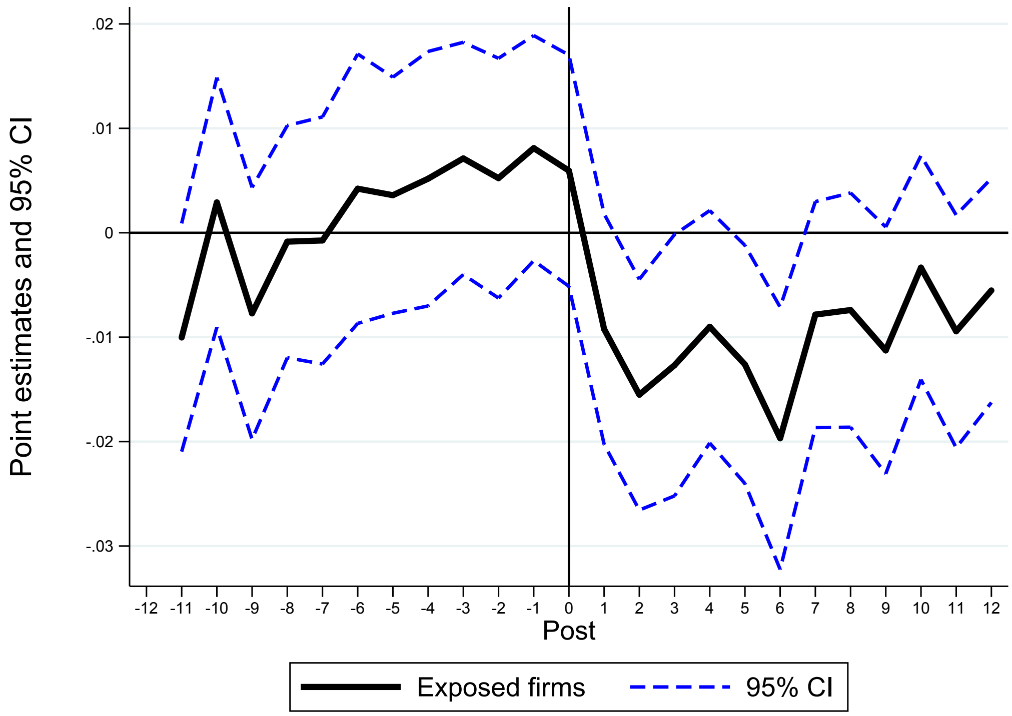
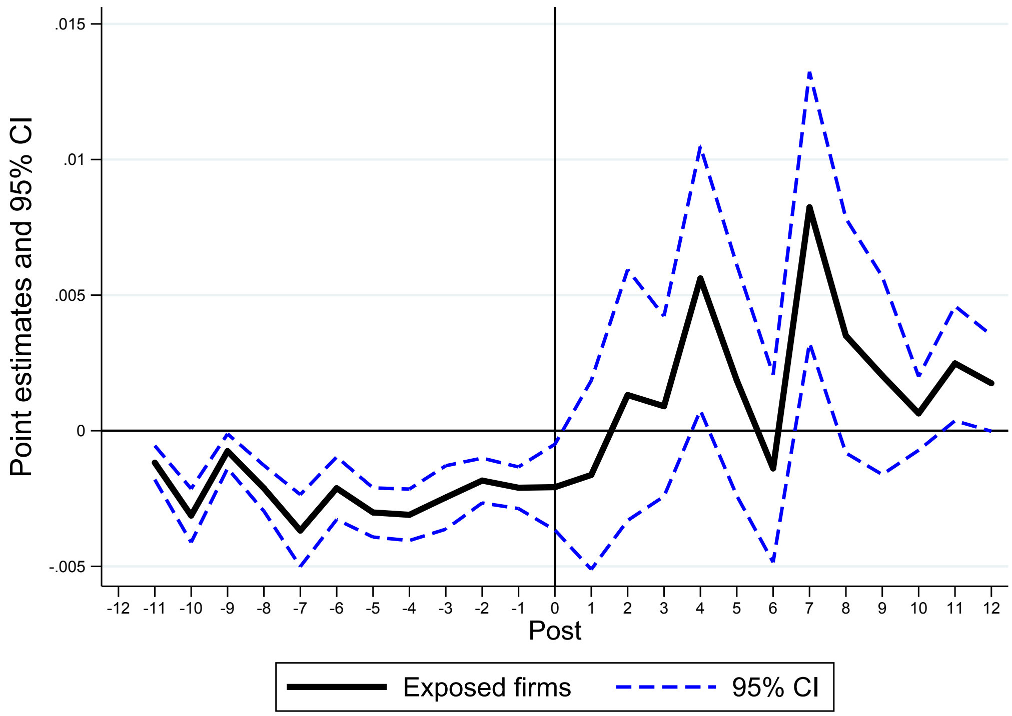


Figure 2.5: Effect of Exposure on Exit



2.5.2 Tables

Table 2.1: Irregularities Detected

Type of Irregularity	Percent	Amount Received (R$)	
		Average	Median
Irregularities in Tendering Process	38.24	237,038	14,317
Documentation Issue/Contract Breach	26.50	99,145	6,300
Fake Receipt/Suspicious Expense	20.43	102,610	2,949
Resource Misuse	7.11	153,370	2,175
Overpricing	3.36	330,330	60,413
Phantom Company; Collusion/Unfair Competition	2.49	238,848	55,374
Poor/Inadequate Quality	1.86	283,326	82,420

Notes: Authors' calculations from manually extracted information contained in audit reports.

Table 2.2: Top-10 Ministries Source of Misused Federal Funds

Ministry	Percent
Ministry of Health	35.83
Ministry of Education	33.14
Ministry of Social Development and Fight Against Hunger	15.97
Ministry of Cities	3.61
Ministry of Rural development	3.31
Ministry of National Integration	2.39
Ministry of Tourism	1.64
Ministry of Sport	1.40
Ministry of Social Security	0.46
Ministry of Environment	0.37

Notes: Authors' calculations from manually extracted information contained in audit reports.

Table 2.3: Top-10 Federal Spending Programs Source of Misused Federal Funds

Program	**Percent**
Schooling Brazil	22.65
Basic Assistance in Health	16.48
Pharmaceutical Assistance	8.40
Basic Social Protection	4.60
Conditional Transfer of Income - Bolsa Família	4.40
Appraisal of Education Professionals	3.77
Urban Water and Sewage Services	3.77
Eradication of Child Labor	3.71
Basic Assistance Bloc-Financial Resources	2.23
Support to Small Cities' Urban Development	1.70

Notes: Authors' calculations from manually extracted information contained in audit reports.

Table 2.4: Exposed and Non-Exposed Firms by Size Category

Size	**Non-Exposed**	**Exposed**
Zero	30.56%	7.67%
Micro (1-4)	43.23%	37.01%
Micro (5-9)	14.77%	19.21%
Small	9.28%	25.51%
Medium	1.34%	4.55%
Large	0.81%	6.05%

Notes: Source: RAIS, Authors' calculations.

Table 2.5: Summary Statistics

	Panel A: Firm Characteristics							
	Exposed Firms				Non-Exposed Firms			
Variables	**N**	**p50**	**Mean**	**Std Dev.**	**N**	**p50**	**Mean**	**Std Dev.**
Number of Employees	10,864	6	53	347	111,589,240	2	12	341
Total Wage Bill (R$)	10,864	3,668	92,005	1,105,297	111,589,240	1,269	15,713	634,030
Average Wage per Employee (R$)	10,082	618	769	713	97,387,540	586	748	723
Log Employment	10,082	1.9	2.1	1.6	97,387,540	1.1	1.1	1.2
Log Total Wage Bill	10,064	8.4	8.6	1.8	97,033,442	7.4	7.7	1.5
Number of (unique) Firms	8,854				7,124,669			

	Panel B: Workers' Characteristics							
	Exposed Firms				Non-Exposed Firms			
Variables	**N**	**p50**	**Mean**	**Std Dev.**	**N**	**p50**	**Mean**	**Std Dev.**
Education	572,366	7	6.3	2	1,324,042,033	7	6.2	2
Gender	572,366	0	0.33	0.47	1,324,042,033	0	0.41	0.49
Age	572,366	34	35	9.6	1,324,042,033	33	34	9.8
Tenure (in Months)	572,366	34	69	83	1,324,042,033	29	60	75
Number of (unique) Workers	444,065				84,747,871			

Notes: The table reports descriptive statistics at firm-level (Panel A) and worker-level (Panel B). The data refers to the month before exposure. Exposed firms are firms that have been exposed at any point in time during the period under study (2003-2014). Non-exposed firms are firms that were never exposed by audit reports during the period under study. Source: RAIS, Authors' calculations.

Table 2.6: Exposed and Non-Exposed Firms by Sector of Operation

Sector	**Non-Exposed**	**Exposed**	**Difference**
Retail Trade and Repairs	30.25%	33.80%	-3.55%
Construction	6.08%	22.80%	-16.72%
Automotives and Fuels Trade	5.18%	10.79%	-5.61%
Wholesale Trade	4.36%	10.41%	-6.05%
Corporate Services	6.17%	2.53%	3.64%
Ground Transportation	3.17%	2.33%	0.84%
Health and Social Services	4.55%	2.23%	2.32%
Public Administration, Defense, and Social Security	0.30%	2.04%	-1.74%
Printing	0.66%	1.47%	-0.81%
Associative Activities	1.67%	1.21%	0.46%
Food and Beverages Manufacturing	1.51%	1.17%	0.34%
Accommodation and Food	5.88%	0.97%	4.91%
Non-Metallic Mineral Manufacturing	0.62%	0.74%	-0.12%
Rentals	0.78%	0.69%	0.09%
Clothing Manufacturing	1.56%	0.50%	1.06%
Machinery and Equipments Manufacturing	0.64%	0.48%	0.16%
Education	1.71%	0.47%	1.25%
Metal Products Manufacturing	1.07%	0.47%	0.60%
Furniture Manufacturing	0.78%	0.42%	0.36%
Non-Metallic Mineral Extraction	0.20%	0.35%	-0.15%
Agriculture	9.58%	0.33%	9.25%
Computed and Related Activities	1.01%	0.32%	0.69%
Chemical Manufacturing	0.30%	0.30%	0.00%
Sewage Cleaning	0.10%	0.29%	-0.19%
Mail and Telecommunication Services	0.46%	0.27%	0.19%
Recreational, Cultural and Sports Activities	1.20%	0.26%	0.94%
Vehicles Manufacturing	0.14%	0.26%	-0.12%
Wood Manufacturing	0.47%	0.23%	0.24%
Rubber and Plastic Manufacturing	0.36%	0.21%	0.15%
Medical Instruments/Equipment Manufacturing	0.14%	0.21%	-0.07%
Auxiliary Transportation Services	0.97%	0.20%	0.77%
Electricity and Gas Distribution	0.07%	0.19%	-0.12%
Textile Manufacturing	0.40%	0.15%	0.25%
Real Estate	2.94%	0.11%	2.83%
Paper Manufacturing	0.13%	0.10%	0.03%
Financial Services	0.63%	0.08%	0.55%
Leather and Footwear Manufacturing	0.40%	0.08%	0.32%
Electrical Machinery/Equipment Manufacturing	0.17%	0.08%	0.09%
Insurance	0.12%	0.08%	0.04%
Water Treatment and Distribution	0.04%	0.08%	-0.04%
Personal Services	1.42%	0.07%	1.35%
Computer and Office Equipment Manufacturing	0.03%	0.05%	-0.02%
Metallurgy	0.15%	0.04%	0.11%
Other Transportation Equipment Manufacturing	0.05%	0.04%	0.01%
Air Transportation	0.03%	0.03%	0.00%
Forestry	0.32%	0.03%	0.29%
Recycling	0.08%	0.03%	0.05%
Auxiliary Financial Services	0.39%	0.02%	0.37%
R&D	0.03%	0.02%	0.01%
Fishing	0.09%	0.01%	0.08%
Oil Extraction	0.01%	0.01%	0.00%
Domestic Services	0.48%	0.00%	0.48%
Electronics Manufacturing	0.05%	0.00%	0.05%
Marine Transportation	0.03%	0.00%	0.03%
Metallic Mineral Extraction	0.02%	0.00%	0.02%
International Organizations	0.01%	0.00%	0.01%
Oil Refining, Production of Nuclear Fuels and Alcohol	0.01%	0.00%	0.01%
Coal Extraction	0.01%	0.00%	0.01%
Tobacco Manufacturing	0.00%	0.00%	0.00%

Notes: Sector of operation is observed in the month of the lottery. Sector classification is 2-digit CNAE.

Table 2.7: Employment Growth

	Panel A: 1-Year Horizon		
	(1)	**(2)**	**(3)**
	10% Random	**Sector and Municipality Match**	**Sector, Municipality Size Match**
$I_i^{Exposed} \times$ Post	-0.0111***	-0.00313*	-0.00388**
	(0.00187)	(0.00181)	(0.00184)
Post	-0.0153***	-0.0169***	-0.0160***
	(0.0001)	(0.0001)	(0.0001)
Firm Fixed Effects	Yes	Yes	Yes
Municipality × Time Fixed Effects	Yes	Yes	Yes
Industry × Time Fixed Effects	Yes	Yes	Yes
Observations	53,443,232	24,313,668	10,903,455
Adjusted R^2	0.098	0.085	0.102
	Panel B: 2-Year Horizon		
	(1)	**(2)**	**(3)**
	10% Random	**Sector and Municipality Match**	**Sector, Municipality Size Match**
$I_i^{Exposed} \times$ Post	-0.0122***	-0.00520***	-0.00554***
	(0.00160)	(0.00153)	(0.00155)
Post	-0.0139***	-0.0155***	-0.0145***
	(0.0001)	(0.0001)	(0.0001)
Firm Fixed Effects	Yes	Yes	Yes
Municipality × Time Fixed Effects	Yes	Yes	Yes
Industry × Time Fixed Effects	Yes	Yes	Yes
Observations	75,204,311	33,771,664	15,292,483
Adjusted R^2	0.088	0.078	0.091
Mean Employment Growth	0.009	0.011	0.011
Std. Dev. Employment Growth	0.146	0.156	0.155

Notes: The following table reports the effect of audit and exposure on the firm's employment growth. In Panel A, the sample covers the window [-12, 12] and in Panel B the period [-12, 24] around the audit month, in monthly unit. The sample excludes government firms, firms with less than five employees, and firms in *any* audited municipalities. The control group for each lottery in the first column was chosen by randomly selecting ten percent of the non-exposed firms at the tome of the lottery. In the second column, for each exposed firm I use as control group all the firms in the same municipality and sector that exist at the month of the lottery. The third column in addition to sector and municipality matches on size category. I then normalize time for both the treated and control group and follow firms before and after the audit. The outcome variable is defined as the log change in total monthly employment. Standard errors are clustered at the firm level.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2.8: Exit

	Panel A: 1-Year Horizon		
	(1)	**(2)**	**(3)**
	10% Random	**Sector and Municipality Match**	**Sector, Municipality Size Match**
$I_i^{Exposed} \times$ Post	0.00450***	0.00248***	0.00269***
	(0.0008)	(0.0008)	(0.0008)
Post	0.00806***	0.00848***	0.00817***
	(0.0001)	(0.0001)	(0.0001)
Firm Fixed Effects	Yes	Yes	Yes
Municipality × Time Fixed Effects	Yes	Yes	Yes
Industry × Time Fixed Effects	Yes	Yes	Yes
Observations	53,727,322	24,454,005	10,965,681
Adjusted R^2	0.133	0.128	0.142
	Panel B: 2-Year Horizon		
	(1)	**(2)**	**(3)**
	10% Random	**Sector and Municipality Match**	**Sector, Municipality Size Match**
$I_i^{Exposed} \times$ Post	0.00586***	0.00382***	0.00393***
	(0.0007)	(0.0007)	(0.0007)
Post	0.00761***	0.00803***	0.00769***
	(0.0001)	(0.0001)	(0.0001)
Firm Fixed Effects	Yes	Yes	Yes
Municipality × Time Fixed Effects	Yes	Yes	Yes
Industry × Time Fixed Effects	Yes	Yes	Yes
Observations	75,523,163	33,928,375	15,362,358
Adjusted R^2	0.129	0.122	0.133
Mean Exit Rate	0.004	0.005	0.005
Std. Dev. Exit Rate	0.067	0.070	0.069

Notes: The following table reports the effect of audit and exposure on the firm's survival. In Panel A, the sample covers the window [-12, 12] and in Panel B the period [-12, 24] around the audit month, in monthly unit. The sample selection and the control/treatment group selections are performed as in Table 2.7. Exit is a dummy variable that equals one if the firm exits in the month. Standard errors are clustered at the firm level.
Significance Levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2.9: Firm Size Dispersion Regressions

	(1)
	$St.Dev.\left[\log\left(\frac{L_{ijt}}{L_{jt}}\right)\right]_{mt}$
$Post_p$	0.006
	(0.004)
$Audited_m \times Post_p$	-0.011**
	(0.005)
Municipality Fixed Effects	Yes
Time Fixed Effects	Yes
Observations	199,731
Adjusted R^2	0.701
No. Municipalities	3,505

Notes: Standard errors clustered at municipality level reported in parenthesis. Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.10: Separations Regressions

	(1)	(2)	(3)
	Separation_{eijt} = {0,1}		
Variables	**3 Months**	**6 Months**	**12 Months**
Post	0.027***	0.027***	0.020***
	(0.003)	(0.003)	(0.002)
Employee Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes
Observations	6,965,475	12,918,987	24,286,869
Adjusted R^2	0.302	0.236	0.200

Notes: Standard errors clustered at firm level reported in parenthesis.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.11: Composition of Exposed Firms' Workers' Characteristics

	(1)	(2)
	Workers' Characteristics	
	At Time of Exposure	**Separated Workers**
Gender		
Male	0.59	0.72
Female	0.41	0.28
Education		
Basic	0.37	0.47
High-School	0.38	0.38
Higher	0.25	0.15
Age		
16-25	0.16	0.24
26-35	0.33	0.39
36-45	0.29	0.24
46-55	0.22	0.14
Contract Type		
Indefinite	0.98	0.94
Fixed	0.02	0.06
Position Type		
Non-Managerial	0.79	0.83
Managerial	0.21	0.17

Notes: Column (1) reports the composition of workers' characteristics at the time of exposure. Column (2) reports the composition of characteristics of the workers that left the firm at any point in time within the 12-month period after the exposure. Exposed firms are from manually extracted dataset constructed from audit reports. Source: CGU and RAIS, Authors' calculations.

Table 2.12: Separations Regressions by Gender

	(1)	(2)	(3)
	$\textbf{Separation}_{eijt}$ = {0,1}		
Variables	**3 Months**	**6 Months**	**12 Months**
Post × Gender	-0.013***	-0.008***	-0.005***
	(0.003)	(0.003)	(0.002)
Employee Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes
Observations	6,965,475	12,918,987	24,286,869
Adjusted R^2	0.302	0.237	0.200

Notes: Standard errors clustered at firm level reported in parenthesis.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.13: Separations Regressions by Education

	(1)	(2)	(3)
	Separation$_{eijt}$ **= {0,1}**		
Variables	**3 Months**	**6 Months**	**12 Months**
Post × Education (2-1)	-0.020***	-0.015***	-0.008***
	(0.003)	(0.003)	(0.002)
Post × Education (3-1)	-0.026***	-0.021***	-0.012***
	(0.004)	(0.004)	(0.003)
Employee Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes
Observations	6,965,475	12,918,987	24,286,869
Adjusted R^2	0.303	0.237	0.200

Notes: Standard errors clustered at firm level reported in parenthesis.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.14: Separations Regressions by Age

	(1)	(2)	(3)
	$\textbf{Separation}_{eijt}$ = {0,1}		
Variables	**3 Months**	**6 Months**	**12 Months**
Post × Age (2-1)	-0.014***	-0.012***	-0.011***
	(0.003)	(0.004)	(0.003)
Post × Age (3-1)	-0.021***	-0.018***	-0.015***
	(0.004)	(0.005)	(0.004)
Post × Age (4-1)	-0.019***	-0.015***	-0.014***
	(0.005)	(0.005)	(0.004)
Employee Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes
Observations	6,965,475	12,918,987	24,286,869
Adjusted R^2	0.302	0.236	0.200

Notes: Standard errors clustered at firm level reported in parenthesis.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.15: Separations Regressions by Hierarchy

	(1)	(2)	(3)
	$\textbf{Separation}_{eijt}$ = {0,1}		
Variables	**3 Months**	**6 Months**	**12 Months**
Post × Manager	-0.008**	-0.006*	-0.003*
	(0.004)	(0.003)	(0.002)
Employee Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes
Observations	6,965,475	12,918,987	24,286,869
Adjusted R^2	0.302	0.236	0.200

Notes: Standard errors clustered at firm level reported in parenthesis.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.16: Exposed Firms' Workers' Reallocation Characteristics

	(1)	(2)	(3)
	Labor Reallocation		
	t = {-12, 0} Outcomes at t = 0	**t = {0, +12}** Outcomes at t = +12	**t = {0, +24}** Outcomes at t = +24
Size			
Larger	0.04	0.04	0.06
Same	0.22	0.21	0.29
Smaller	0.09	0.10	0.17
Unemployed	0.65	0.65	0.48
Industry			
Same	0.54	0.50	0.46
Different	0.46	0.50	0.54
Municipality			
Same	0.51	0.49	0.49
Different	0.49	0.51	0.51
Exposed Firms			
Exposed	0.17	0.09	0.05
Non-Exposed	0.83	0.91	0.95

Notes: The table reports characteristics of destination firms for workers that leave exposed firms. Column (1) refers to labor outcomes up to the month of exposure for workers that were employed at the exposed firm 12 months before the exposure and left at any point in time between this month (t = -12) and the month of exposure. Column (2) refers to labor outcomes up to 12 months after the exposure for workers that were employed at the exposed firm at the time of exposure and left at any point in time between the month of exposure and 12 months after. Column (3) refers to labor outcomes up to 24 months after the exposure for workers that were employed at the exposed firm at the time of exposure and left at any point in time between the month of exposure and 12 months after.

Chapter 3

Family Control and the Cost of Debt: Evidence from the Great Recession

3.1 Introduction

Founders or their families control the majority of firms around the world (Faccio and Lang (2002)). Even among public firms, families control 45% of the listed international firms (La Porta, Lopez-de Silanes, and Shleifer (1999)) and at least one third of S&P500 firms (Anderson and Reeb (2003)). Despite the prevalence of family firms, we have limited knowledge on how family control affects firm policies and outcomes (Bennedsen, Pérez-González, and Wolfenzon (2010)). Prior research has mainly focused on implications of family control for equity valuation. The relationship between family control and the agency cost of debt has received limited attention, although debt is a major source of external finance for firms. Theoretical predictions on how family control should affect agency cost of debt are actually ambiguous, and empirical measurement has been difficult due to the endogeneity of family control. In this essay, I measure the direction and magnitude of the effect of founding-family control on the cost of bank debt. To overcome the endogeneity problem, I exploit the recent financial crisis and the unexpected nature of Lehman Brothers' collapse as a natural laboratory in order to tease out the effect of family control.

Theoretically, predicting how family control affects the cost of debt is difficult because we must consider different dimensions. On one hand, family control can exacerbate agency conflicts and increase the cost of debt. The existence of a controlling shareholder elevates the risk of strategic default (Hart and Moore (1994), Bolton and Scharfstein (1996), Hart and Moore (1998)). Furthermore, the family as a dominant shareholder can influence post-default restructuring and extract some of the surplus from creditors (Aslan and Kumar (2012)). Moreover, the controlling family can extract private benefits at the expense of the firm's other constituents. Family firms are also more opaque than diffused shareholder firms (Anderson, Duru, and Reeb (2009), Fan and Wong (2002)), which makes fraud more likely. On the other hand, family control might mitigate shareholder-debtholder conflicts and decrease the cost of debt. The family has a long-term commitment to the firm, spanning various generations, and its reputation is tied to the firm. Moreover, the family has a large and highly undiversified investment in the firm. The combination of the long-term commitment, the reputation concerns, and the high, undiversified cash-flow stakes suggest the family is more likely to value firm survival over strict wealth maximization. Thus, how family control should affect the cost of debt in practice is theoretically ambiguous.

Although limited, a number of empirical studies have explored the relationship between family control and cost of debt for different debt instruments. However, the evidence remains mixed. The earlier work in this area finds family firms are related to a lower cost of debt. The first paper that looks into this question is Anderson, Mansi, and Reeb (2003), who look into public debt for 252 S&P 500 firms and find family firms are associated with a lower cost of public-debt financing. Ellul, Guntay, and Lel (2007) look at international bond issues and find the relationship between family control and cost of debt varies with the level of investor protection. Family firms in low-investor-protection countries suffer from higher public debt costs, whereas family firms in high-investor-protection environments benefit from lower public-debt costs.

On the other hand, recent papers have called into question the negative relationship between family control and the cost of debt. In terms of private debt agreements, in an international setting, Aslan and Kumar (2012) find family firms are associated with a higher agency cost of debt. Also in an international setting, Lin, Ma, Malatesta, and Xuan (2011) find the higher the wedge between control and cash-flow rights, the higher the cost of debt, especially for family firms. The effect is further amplified if the CEO is a member of the controlling family.

Despite the various evidence, establishing a causal effect of the family on the cost of debt is difficult due to endogeneity concerns. The main challenge is that a third omitted factor could drive both family control and cost of debt. In this essay, I measure the direction and magnitude of the effect of founding-family control on the cost of bank debt, by taking advantage of the recent financial crisis and the unexpected nature of Lehman Brothers' collapse as a natural laboratory in order to tease out the effect of family control.

For my empirical analysis, I focus on private credit agreements in the syndicated loan market for the years 2004-2010. I employ a difference-in-differences approach and compare the change in spreads on syndicated loans during the crisis between family and non-family firms. As Lemmon and Lins (2003) point out, financial crises represent a relative exogenous shock, with respect to the individual firm. One potential concern, however, is that family and non-family firms might have differential exposure to the shock. To alleviate any potential concerns (e.g., due to family firms matching with better financial institutions), I further exploit the cross-sectional dispersion in lender health induced by the collapse of Lehman Brothers as a source of exogenous variation in the exposure to the shock. Specifically, I hypothesize that if financial frictions make accessing external capital or switching from one source of capital to another difficult, firms that maintained lending relationships with financial institutions that were highly exposed to the negative liquidity shock during the crisis experienced tighter financial constraints and a higher cost of accessing the syndicated loan market. Therefore, the research design I consider compares firms with different levels of control at the same point in time that have been subject to the same shock and have similar exposure to the shock based on their lending relationships with differentially liquidity-struck financial institutions.

I find the increase in loan spreads around the Lehman crisis was at least 24 basis points lower for family firms. Furthermore, the gap in spreads among family and non-family firms becomes wider among firms that had pre-crisis relationships with less healthy lenders. Specifically, in the group of firms that were highly exposed to the Lehman collapse, the spreads on loans family firms took out during the crisis were at least 73 basis points lower compared to the spreads on loans non-family firms took out. This difference is sizeable given that the mean spread in the crisis period was 344 basis points. The results hold when I look separately into term loans and credit lines. The analysis shows family control is associated with a lower cost of debt during the Great Recession and the effect of family ties is exacerbated among firms with higher exposure to the liquidity shock. The results provide evidence that family control has a greater effect on the cost on borrowing, especially when lenders are constrained.

Furthermore, I investigate other contract terms. I find no significant difference in loan maturity, but I find some evidence that family firms received higher loan amounts during the crisis. I also look into the extensive margin but find no difference between family and non-family firms in the access to the bank-lending market during the crisis.

I provide numerous tests that show the robustness of my results. I show that differences in corporate policies related to dividends, cash holdings, and leverage between family and non-family firms during the crisis do not explain my results. Furthermore, the results are similar when I repeat the analysis in a subsample in which I require firms not only to have accessed the bank-lending market before and after the crisis, but also to have borrowed the same type of loan from the same lender. Moreover, I use a matching-estimator approach to alleviate concerns that some pre-crisis characteristics might differ across the family and non-family group of firms, and these characteristics potentially explain both the founding family's endogenous decision to maintain control in the firm and to secure bank financing in the crisis. I also provide evidence that differential changes in credit quality for family and non-family firms during the crisis or the selection of firms that get a loan during the crisis do not drive my results. Finally, my results are robust to using an alternative definition for exposure to the liquidity shock based on Chodorow-Reich (2014).

I next move to consider potential alternative interpretations of my results. One potential alternative interpretation is that family-controlled firms maintain a longer and tighter relation with financial institutions before the shock, and thus, the difference in the length of the lending relations between family and non-family firms rather than the family control itself might drive the result. Furthermore, another alternative explanation that may be potentially responsible for my results is that lenders may not value family control per se in the presence of liquidity constraints, but, in general, value the presence of a blockholder, thus leading to a lower cost of bank capital. Moreover, a potential concern is that founder-run firms that might not be family firms drive my results. Finally, I also examine whether family-controlled firms experience a lower cost in accessing bank financing, because they accept stricter covenants in the lending agreements. I test and find no support for these alternative interpretations.

Having established the relationship between family control and cost of debt, I proceed with unveiling potential (non-mutually) exclusive factors that influence the relation between family control and loan spreads. I find the gap in spreads between family and non-family firms is higher in firms with higher expected shareholder-debtholder agency conflicts. Furthermore, as previous literature has stressed the effect of a family member acting as the CEO on firm performance, I explore the impact of CEO affiliation on the cost of debt within the subsample of family firms, and find credit spreads are lower when family CEOs run the firms. Furthermore, I provide novel evidence that for 17% of the family firms, creditors impose explicit restrictions in private credit agreements that require the founding family to maintain a minimum percentage of ownership or voting power. These types of covenants show creditors value the involvement of the founding family. Finally, I investigate covenant violations and find no difference in covenant violations between family and non-family firms.

My results make several contributions to the literature. By taking advantage of the recent financial crisis and the unexpected nature of Lehman Brothers' collapse as a natural laboratory, I am able to tease out the effect of family control on the cost of private debt.[1] Although I cannot randomly assign family control to firms, the research design I consider compares family and non-family firms at the same point in time that have been subject to the same shock, and observe the effect on their debt cost. Moreover, in my empirical design, I compare family with non-family firms with similar exposure to the shock, further alleviating concerns that family firms might have different exposure to the shock through their lenders.[2] Thus, my identification strategy allows us to add to the prior literature that examines the link between control and the cost of debt (Lin, Ma, Malatesta, and Xuan (2011), Aslan and Kumar (2012)) and in particular the literature on family control and cost of debt (Anderson, Mansi, and Reeb (2003), Ellul, Guntay, and Lel (2007)), by establishing a causal link between family control and cost of debt and providing robust evidence on the direction and magnitude of the impact. Because my essay focuses on the United States, it is more closely related to Anderson, Mansi, and Reeb (2003), which is the first paper that studies the relationship between family control and cost of debt. I add to their findings not only by being able to establish a causal link between family control and the cost of debt, but also by focusing on private debt agreements. Eighty percent (80%) of public firms in the United States have private credit agreements, compared to only 15%-20% that have public

[1]Furthermore, by conducting the analysis within a country I avoid the potential concerns arising due to unobserved differences among various countries (Mayda and Rodrik, 2005).

[2]Thus, my empirical design improves upon prior literature that compares firms with different control structure around crises (Lins, Volpin, and Wagner (2013), Lin, Ma, Malatesta, and Xuan (2011)) by addressing concerns of potential differential exposure to the shock for firms with different control structure.

debt (Nini, Smith, and Sufi (2009)). Also my results add to the evidence of Aslan and Kumar (2012) who focus on private credit agreements in an international setting. They find that family firms are associated with higher cost of debt, while I find the opposite effect in US.

Furthermore, to my knowledge, this essay is the first to document that agency cost of debt of family control becomes lower during financial shocks. Furthermore, I show lender constraints exacerbate the effect of family ties. Lins, Volpin, and Wagner (2013) provide international evidence that family control negatively affects minority shareholders during the 2008 crisis, but they find no evidence on how family control affects agency cost of debt. My results allow me to better understand how family control affects firm policies and outcomes. The findings, thus, more generally contribute to the literature that investigates how family control affects firm valuation (e.g., Anderson and Reeb (2003), Villalonga and Amit (2006), Ellul, Guntay, and Lel (2007), Bennedsen, Nielsen, Perez-Gonzalez, and Wolfenzon (2007), Anderson, Duru, and Reeb (2009), Masulis and Mobbs (2011)).[3]

3.2 Methods

3.2.1 Sample Construction

To construct my sample, I collect information on firms that are present in both the Thompson Reuters Dealscan database and Compustat. I connect the two databases using the linking table based on the information provided in Chava and Roberts (2008). The Thompson Reuters Dealscan database contains information on syndicated loans. The data that are available comprise the identities of the borrowing entity and the lending institution that

[3]See Bennedsen, Pérez-González, and Wolfenzon (2010) for a comprehensive survey.

participated in the deal at origination, the terms of the loan, and the purpose of the loan.

I focus on non-financial US borrowers. That is, I exclude borrowers with SIC codes 6011-6799, and I require that each deal has information on the interest spread of all tranches in the deal. I restrict my main sample to borrowers that have obtained a syndicated loan both in the pre-crisis period (January 2004-September 2008) and the post-crisis period following the collapse of Lehman Brothers (October 2008-December 2010).[4]

For the purpose of exploring the heterogeneous response to the shock based on the type of lending facility, I classify a loan as a term loan if Dealscan explicitly reports the loan type to be a term loan (e.g., Delay Draw Term Loan, Term Loan A, Term Loan B), whereas I classify a loan as a credit line if Dealscan reports the loan type to be one of the following: 364-Day Facility, Revolver/Line $<$ 1 Yr., Revolver/Line $\geq$ 1 Yr., Demand Loan. To identify a syndicate's lead financial institution, I follow Ivashina (2009). Specifically, I define the administrative agent to be the lead bank if identified; otherwise, I define the financial institutions that act as agent, arranger, bookrunner, lead arranger, lead bank, or lead manager to be lead banks. I corroborate and complement the information on the role of a participant in a syndicate as a lead arranger by taking advantage of the LeadArrangerCredit field available in Dealscan.

Regarding the financial information of lending institutions, if the highest level parent is either a domestic financial holding company or a domestic bank holding company, I collect information related to financial data of lending institutions by hand-matching at the

[4]In Table 3.10 that tests the extensive margin, the sample contains all the borrowers that obtained a syndicated loan in the pre-crisis period, irrespectively of whether they obtained a loan in the post-crisis period.

holding-company level between the name, the geographic location, and the operational period as reported in Dealscan and as presented in the Federal Financial Institutions Examination Council's (FFIEC) National Information Center (NIC) database. For foreign holding companies and investment banks, I collect the relevant information either from Bankscope or based on manual inspection of the financial statements of the financial institutions. To control for mergers prior to the onset of the crisis, the acquiring lenders inherit the target's syndicated lending relationships with both borrowers and other lenders, thus transferring the unexpired loans at the date of the merger to the acquirer's record. For mergers that take place following the collapse of Lehman Brothers, I maintain separate identifiers for the acquiring and the target firm, although in estimating the measure of change in lending supply, I consider as borrowing from the target if a borrower of the target in the pre-crisis period obtains a crisis loan from the acquirer.

My final sample consists of 1,171 firms that have 6,169 lending facilities extended by 71 banks (2,006 bank-firm pairs). The unit of observation is a bank-firm-loan triple. Of the 1,171 borrowers, 313 firms (26.7%) are classified as family firms.

3.2.2 Definition of Family Firms

To identify family firms, I manually collect information on the presence of a founding family from 10-K and DEF14A fillings. I also corroborate the information with corporate histories for each firm in the sample. I collect corporate history information from Factiva, Hoovers, and company press releases. In cases of family presence, I manually collected information on family involvement[5] in boards of directors and in firm management from 10-K and DEF14A fillings.

I follow Anderson and Reeb (2003) and Villalonga and Amit (2006), and classify firms as family firms if the founder or a member of his family by either blood or marriage is an officer, director, or blockholder, either individually or as a group. My main analyses are based on this definition. Furthermore, in subsequent tests, I show the robustness of my results if I impose additional restrictions on the definition of family firms. Specifically, these restrictions include the family being the largest voteholder, the largest shareholder, or in the second generation or later. My later test aims to alleviate concerns that founder-run firms that might not be family firms drive my results.

3.2.3 Measures of Exposure to the Shock

In my analysis, I compare family to non family firms with similar exposure to the shock, to alleviate concerns that my results are driven by family firms having differential exposure to the shock. To estimate the heterogeneous impact of the collapse of Lehman Brothers on borrowing firms, I follow Ivashina and Scharfstein (2010) and construct a firm-level measure that depends on the exposure of each financial institution to Lehman through co-syndication. The measure relies on the assumption that borrower-lender relationships are sticky. Specifically, at the lender level, I estimate the exposure as the fraction of the number of outstanding credit lines co-syndicated with Lehman and in which Lehman is the lead arranger, over the total number of outstanding revolving facilities of the lender at the time of the collapse.

[5] Although the Osiris database from Bureau Van Dijk offers good coverage for international firms' ownership structure, their coverage for US firms is limited, thus I handcollect it.

Then, at the firm level, I focus on the last pre-crisis syndicated loan for each firm i, and I construct a weighted-average exposure measure based on each financial institution b that is part of syndicate s, with weights corresponding to the participation rates of each lender in the syndicate $a_{b,i,last}$. Thus, I estimate the magnitude of exposure to the shock based on the following weighted-average aggregation:

$$LehmanExposure_{i,s} = \sum_{b \in s} a_{b,i,last} LehmanExposure_b. \tag{3.1}$$

The rationale is that the collapse of Lehman Brothers imposed a liquidity problem in the financial institutions with which Lehman maintained a co-syndication relationship, because the exposed financial institutions were forced both to replace the role of Lehman in the syndicated loan and to confront additional drawdowns when maintaining credit lines with the borrowers. Besides, because the impact was heterogeneous among lenders based on the level of exposure to the failing institution, the exogenous variation created is useful in controlling for the differential exposure to the shock at the bank-firm level and in examining the impact on contractual terms.

Moreover, I show my results are robust to using as an alternative measure of exposure to the shock, the measure proposed in Chodorow-Reich (2014) that relies on the heterogeneous change in the lending supply of financial institutions following the Lehman collapse. Specifically, the lending-supply measure is defined based on the difference in the quantity of loans initiated by lender b to all borrowers other than firm i before and after the collapse of Lehman Brothers. The pre-crisis period that is considered for the construction of the measure is the nine-month periods from October 2005 to June 2006 and October 2006 to

June 2007, whereas the crisis period is from October 2008 to June 2009. The loan quantities have been weighted based on the participation rates of the lender in the syndicated loan. Hence, letting $L_{-i,b}$ equal 1 if bank b has a lending relationship with borrower j in period t and letting $a_{b,j,t}$ equal the participation rate of the syndicated loan, the change in the credit supply for a lender-borrower pair is constructed as follows:

$$\Delta L_{-i,b} = \frac{\sum_{j \neq i} a_{b,j,crisis} L_{b,j,crisis}}{0.5 \sum_{j \neq i} a_{b,j,pre-crisis} L_{b,j,pre-crisis}} \tag{3.2}$$

Then, the final measure of bank health corresponding to the last pre-crisis syndicated loan of each borrower is based on the participation rates of each lender of the last pre-crisis syndicated loan and the above measure of change in lending quantities before and after the Lehman collapse:

$$\Delta \tilde{L}_{i,s} = \sum_{b \in s} a_{b,i,last} \Delta L_{-i,b} \tag{3.3}$$

If the actual share of each lender in a loan commitment is missing in Dealscan, I calculate the participation rate as the average share of lead lenders and participants in a facility involving the same structure.

Previous research has examined the effectiveness of my measures, demonstrating a significant contraction in the bank lending channel occurs following the Lehman collapse, the dispersion of which among financial institutions is sufficiently captured by the proposed bank-health measures. In particular, Chodorow-Reich (2014) shows the credit-supply measure is negatively correlated with exposure to Lehman through co-syndication, controlling for a battery of observable bank characteristics, indicating the contraction in bank credit is supply-driven. The use of the alternative measure contributes to mitigating any concerns that the findings potentially are affected by the shuffling of financial institutions in being the lead arrangers in the post-crisis loan originations in the syndicated market. Specifically, as the measure captures the change in the lending supply around the crisis, by using the alternative measure I am able to show that my findings are not driven by family firms being disproportionately matched with banks that increased their share in the post crisis.

3.2.4 Descriptive Statistics

Table 3.1 provides summary statistics for the key variables in the analysis, broken down by family and non-family firms. I find that the percentage of family firms in my sample is 26.7%, which is in line with the percentage of family firms that Villalonga and Amit (2006) and Anderson, Mansi, and Reeb (2003) report for Fortune 500 (37%) and S&P500 (34%) firms, respectively. Panel A of Table 3.1 concentrates on firm characteristics. On average, family firms have fewer assets than non-family firms and are younger, and the difference is statistically significant at the 5% level. The average leverage for family firms is 34%, which is not significantly different from non-family firms. Furthermore, family firms do not differ in terms of cash flow, interest expense, cash holdings, and S&P rating. Therefore, family and non-family firms appear to be balanced among the aforementioned observable characteristics apart from size and age; however, size and age are expected to induce a bias - if any - against my results, because size is negatively correlated with credit spreads, implying non-family firms might experience favorable rates due to larger size or higher age. Nevertheless, I use a matching estimation approach to alleviate any concerns related to alternative sources of firm heterogeneity that underlie the observed relation.

Panel B of Table 3.1 compares the characteristics of loans that family and non-family firms take out before the crisis. The loan-characteristics measures come from Dealscan, which allows me to identify deal-level data and observe the terms of loans at origination. Credit spread is the main dependent variable I use in my analysis. The average spread on loans that family firms take out is 190 basis points and is not significantly different from the average spread for loans that non-family firms take out. Also, when I split the loans into term loans and credit lines, I do not find any statistically significant difference in the spreads between loans that family versus non-family firms take out. Furthermore, the loans that family firms take out do not differ in terms of maturity and average loan amount. Finally, I compare whether pre-crisis family firms were matched with banks that had a higher fraction of their syndicated portfolio co-syndicated with Lehman Brothers, and in which Lehman has a lead role (Lehman exposure). I do not find a difference between family and non-family firms in exposure to Lehman.

Panel C of Table 3.1 provides summary statistics for bank characteristics. In particular, in my specifications, I use lead bank characteristics that are able to capture both the strength of a financial institution to absorb potential losses (e.g., capital ratio) and the liquidity of its funding base (e.g., deposit ratio) as controls. I also use financial performance measures (e.g., profitability) and actual measures of loan portfolio quality that are based on the expectations of the loan portfolio behavior (e.g., provisions for loan losses) as controls. The final sample includes 71 banks. The average bank has $550 million in assets in the pre-crisis period.

Table 3.2 provides information on the distribution of lending facilities per origination year. The purpose of the table is to mitigate concerns that the results might be driven by the borrowing activity of family firms been differentially skewed towards the end of my crisis period - a period of less tightening terms in the provision of credit as a result of Fed's unconventional policy actions to spur economic activity. It is apparent from Table 3.2 that there are no significant differences in the timing that family and non-family firms access the syndicated lending market, thus alleviating concerns that seasonality or potential heterogeneity in credit cycles at the time that family and non-family firms originate lending facilities, introduce any bias in the interpretation of the results.

Table 3.3 shows the distribution of family and non-family firms in my sample, using the Fama-French 48-industry classification. Similar to Villalonga and Amit (2006), I find family firms are present throughout the economy, but the percentage of family firms in the various industries differs. The industries with the highest percentage of family firms are apparel, beer and liquor, personal services, and printing and publishing, whereas defense, fabricated products, gold, metals and mining, and tobacco products have no family firms. The difference in the presence of family firms within and across industries suggests the importance of controlling for industry in all my analyses.

3.3 Results

3.3.1 Family Firms and Credit Spreads: Baseline Results

I start the analysis by studying the effect of family control on loan spreads during the crisis. I first offer graphical evidence. Figure 3.1 depicts the time-series evolution of average credit spreads around the financial crisis for family and non-family firms. Figure 3.1 shows that around the crisis, credit spreads increased for all firms, but the increase was larger for non-family firms. Furthermore, the figure shows the trends of family and non-family firms before the crisis exhibit no noticeable differences. My main analysis that follows, confirms this evidence. In Table 3.4, I explore the change in loan spreads during the crisis for family and non-family firms. Panel A presents the univariate analysis. Column (1) presents mean loan spreads for family firms, Column (2) presents mean spreads for non-family firms, and Column (3) provides the difference in means between the loan spreads for family and non-family firms. Panel A shows that before the crisis there is no statistically significant difference in spreads between family and non-family firms. During the crisis, loan spreads increased by 129.93 basis points for family firms and by 152.12 basis points for non-family firms. Thus, non-family firms experienced a 22.2 basis points higher increase in loan spreads relative to family firms. The difference is not only statistically significant, but also economically significant, because non-family firms experienced a 15% higher increase in spreads.

In Panel B, I provide regression results. The specification I estimate is the following:

$$y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{i,t-1} + \theta Z_{b,t-1} + \eta_b + \eta_s + \varepsilon_{it} \quad (3.4)$$

where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ (either 0 or 1) is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is an indicator variable that takes the value 1 for family firms and 0 for non-family firms. $X_{i,t-1}$ are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. Furthermore, I include controls for bank characteristics $Z_{b,t-1}$. Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. Section 3.2.4 describes in detail the firm and bank controls used in the tests. η_s are industry fixed effects. Finally, I add bank fixed effects η_b to control for time-invariant bank characteristics. In Column (1), I do not include any firm or bank controls, whereas in Column (2), I include firm and bank controls. In both Columns (1) and (2), I include industry fixed effects to absorb any time-invariant industry heterogeneity. Column (1) shows that spreads increased during the crisis for all firms, as reported by the positive coefficient on the variable *Post*. But for family firms, the increase in spreads was 28.96 basis points lower than the increase in spreads for non-family firms. The results are similar in Column (2), where I introduce firm and bank controls. In Column (3), I add firm fixed effects to account for firm time-invariant heterogeneity. I do not include I_i^{Family} as a stand-alone variable in the model because it is subsumed by the firm fixed effects. The coefficient on $Post \times I_i^{Family}$ remains both economically and statistically significant when I add firm fixed effects. The results in Table 3.4 indicate family firms experienced a smaller increase in the credit spreads relative to non-family firms, and family control seems to affect the agency cost of debt.

3.3.2 Family Firms, Credit Spreads and Exposure to the Shock

The previous subsection showed that family control is associated with lower agency costs of debt. One potential concern is that family and non-family firms might have different exposure to the liquidity shock through their banks. Addressing this concern is especially important as Table 3.19 and prior literature (Santos (2010)) shows that firms maintaining lending relationships with financial institutions with high exposure to Lehman experienced a higher increase in interest rates during the crisis.

To address this concern, I split the sample into firms that experienced a larger exposure to the liquidity shock and firms with limited exposure, and re-run the specification (3.4) that focuses on the impact of family control. I classify a firm as having high exposure to the shock if the measure of exposure to the Lehman collapse ($LehmanExposure_i$) is in the top 25% of the distribution, and as having low exposure to the shock if the measure of exposure to the Lehman collapse is in the bottom 25% of the distribution. The analysis in Table 3.5 allows me to isolate the impact of family control within a particular level of exposure,thus mitigating the concern that the results are driven by family-controlled firms having less exposure to the crisis if they were matching with financial institutions that engaged less in co-syndication activity with Lehman. Furthermore, the analysis in Table 3.5 allows me to capture a potential heterogeneous impact of family control between different exposure levels to the shock.

The results in Table 3.5 show that firms in both subsamples experienced a higher cost in accessing the bank lending market, as reported by the positive coefficient on the variable *Post*, but as expected, the increase in the cost of bank debt was higher for firms with the highest exposure to the liquidity shock though their banks. Furthermore, Table 3.5 unveils an interesting observation. Family control has a statistically and economically significant impact on the cost of acquiring bank credit in the crisis period only for the subsample of firms that maintained lending relationships with financial institutions highly exposed to the collapse of Lehman. Indeed, only family-controlled firms of the highly exposed subsample (Columns

(1) and (2)) experienced lower credit spreads in the lending facilities originated during the crisis, compared to widely held firms. The economic magnitude is in the level of 75 basis points, implying that highly exposed family-controlled firms experience a sizeable benefit in the cost of private debt compared to a mean spread of 344 basis points in the crisis period. In Columns (2) and (4) I add firm fixed effects to control for firm time-invariant heterogeneity. The coefficient on $Post \times I_i^{Family}$ shows the results are robust when I include firm fixed effects. Furthermore, in unreported results I repeat the analysis restricting my sample to the period from 2005 to 2009 to reflect the termination of the post-Lehman recessionary period that ended in 2009 and the results remain unchanged.

Table 3.20 explores the impact of family control on credit spreads in subsamples with similar exposure to the shock, although examining term loans and credit lines separately, and reports similar results. The rationale of splitting my sample based on the type of lending facility is based on the fact that different loan types potentially involve heterogeneous pricing characteristics that the additive nature of my specification in Table 3.5 fails to incorporate. Specifically, Table 3.20 shows that family firms are associated with a 120-123 basis points lower spread for highly-exposed firms in the case of term loans and a 42-47 basis point in the subsample that consists of credit lines.

Table 3.21 repeats the analysis of Tables 3.5 and 3.20 with an alternative triple-difference specification that takes advantage of the continuous variation in the Lehman exposure. The dependent variable is the credit spread on loans. The specification I estimate is the following:

$$
\begin{aligned}
y_{it} =& \alpha + \beta Post_{it} + \gamma LehmanExposure_i + \theta I_i^{Family} + \mu I_i^{Family} \times Post_{it} + \\
& + \nu I_i^{Family} \times LehmanExposure_i + \delta Post_{it} \times LehmanExposure_i + \\
& + \phi Post_{it} \times LehmanExposure_i \times I_i^{Family} + \zeta X_{i,t-1} + \chi Z_{b,t-1} + \eta_{bi} + \eta_s + \varepsilon_{it}.
\end{aligned}
\tag{3.5}
$$

The specification is the same as that in equation (3.4), but I now introduce the interactions of I_i^{Family} with the variables $LehmanExposure$ and $Post$. All the specifications include firm and bank controls, as well as bank and industry fixed effects. In Column (1), I investigate credit spreads on all loans taken out. In Columns (2) and (3), I present results separately for term loans and credit lines.

The main coefficient of interest in Column (1) is the coefficient on the triple interaction $Post \times I_i^{Family} \times LehmanExposure$. The negative coefficient shows that among the firms that had high exposure to the liquidity shock, family firms got a lower credit spread compared to non-family firms in their loans taken out during the crisis. In terms of economic significance, a standard-deviation increase in the exposure to the liquidity shock is associated with a 26 basis points lower cost of bank capital for family firms compared to non-family firms. In Columns (2) and (3), I repeat the analysis separately for term loans and credit lines and find similar results. Specifically, a standard-deviation increase in the exposure to the liquidity shock is associated with a 32 basis points lower spread for family firms in the case of term loans and a 22 basis point in the case of credit lines.

Therefore, the results show family firms are associated with lower cost of debt and the effect of family ties is exacerbated especially among firms with higher exposure to the liquidity shock. If financial frictions make it difficult for firms to access external capital or switch from one source of capital to another, firms that maintained lending relationships with financial institutions that were highly exposed to the negative liquidity shock during the crisis experienced tighter financial constraints. The results provide evidence that family control has a particularly greater effect on the cost on borrowing among the firms with higher exposure to the shock, potentially because agency conflicts become binding in the highly affected group. In section 3.3.6, I will further investigate various potential factors that can drive the effect of family control and analyze whether the lower debt cost is concentrated in family firms expected ex ante to have larger agency costs.

3.3.3 Other Contract Terms and Access to the Bank Lending Market

In Table 3.6, I examine the change in loan maturity and loan amount for loans taken out before and after the crisis for family and non-family firms. The specification I estimate is similar to equation (3.5), but now the dependent variable is either loan maturity or the log of the loan amount. All the specifications include industry and bank fixed effects. In Columns (1) and (2), I investigate loan maturity, and in Columns (3) and (4), the log of loan amount. The negative coefficient on the variable *Post* in Columns (1) and (2) indicates loan maturity decreased by about 10-12 months during the Great Recession period, whereas the negative coefficient on the variable *Post* in Column (4) provides evidence in support of a decline in the amount of lending in the crisis period. Furthermore, the coefficient on the interaction $Post \times I_i^{Family}$ is insignificant in both cases in the case of maturity, which shows no difference in the maturity of loans taken out by family and non-family firms during the crisis, whereas

I find limited evidence that family firms have also accessed a larger pool of funds during the crisis period. This table alleviates a potential concern that the effect of family firms on credit spreads that I find during the financial crisis is due to lower maturity or loan amount.

Table 3.7 focuses on the extensive margin and examines whether family-controlled firms have heterogeneous access to the bank lending market during the crisis, by considering a probit specification. For this test, the sample contains all the borrowers that obtained a syndicated loan in the pre-crisis period (3,322 firms and, out of these, 733 are family firms). The dependent variable is an indicator corresponding to whether the borrower who had obtained a loan in the pre-crisis period obtained a new loan commitment between October 2008 and December 2010. The results indicate no differences between family and non-family firms in accessing the bank lending market in the crisis period.

3.3.4 Robustness Tests

In this section I provide numerous tests that show the robustness of my results. I show that my results are not explained by differences in corporate policies related to dividends, cash holdings, and leverage between family and non family firms during the crisis. Furthermore, the results are similar when I repeat the analysis in a subsample in which I require firms not only to have accessed the bank lending market before and after the crisis, but also to have borrowed the same type of loan from the same lender. Moreover, I use a matching estimator approach to alleviate concerns that some pre-crisis characteristics might differ across the family and non-family group of firms, and these characteristics potentially explain both the founding family's endogenous decision to maintain control in the firm and to secure bank financing in the crisis. I also provide evidence that my results are not driven by differential changes in credit quality for family and non-family firms during the crisis or by the selection of firms that get a loan during the crisis. Finally, my results are robust to using an alternative definition for exposure to the liquidity shock.

3.3.4.1 Differences in Corporate Policies

The purpose of my essay is to examine the relation between family control and the contract terms associated with syndicated lending. Treating the recent financial crisis as an exogenous shock, I find that family-controlled firms exhibit lower costs of accessing the syndicated loan market in the crisis period. Nevertheless, a potential concern is that the observed relation reflects differences in corporate policies between family-controlled firms and non-family firms during the crisis. For example, as creditors are only interested in the ability of the borrowing firms to repay the loan obligations, if non-family firms distributed a larger amount of dividends to shareholders in the period following the Lehman collapse compared to family-controlled firms or disproportionally accumulated debt, the lending financial institutions are likely to differentially re-evaluate the creditworthiness of non-family firms. Therefore, in Table 3.8, I examine whether family firms engaged in different corporate actions during the crisis relative to non-family firms. Specifically, I investigate their policies regarding dividends, cash holdings, and leverage. The specification I estimate is similar to equation (3.4), but now the dependent variable is the respective financing decision. All the specifications include quarter fixed effects and industry fixed effects. Columns (1)-(4) present the results for firms with high exposure to the collapse of Lehman Brothers, whereas Columns (5)-(8) present the results for firms with low exposure. The coefficients on $Post \times I_i^{Family}$ show family-controlled firms do not differ in their crisis-period decisions about cash holdings, cash, leverage, and short-term debt compared with non-family firms, implying that my findings are not explained by differences in corporate policies during the crisis.

3.3.4.2 Subsample of Borrowers Borrowing from the Same Lender

My previous results show family firms get lower credit spreads than non-family firms during the crisis. Furthermore, I show that firms that had high exposure to the liquidity shock are the primary driver of this difference in spreads. In this section, I repeat the analysis in a subsample in which I require firms not only to have accessed the bank lending market before and after the crisis, but also to have borrowed the same type of loan from the same lender. This subsample alleviates concerns that firms that switch lenders or firms that switched to different types of loans taken out from the same lender drive the results. For example, because revolving facilities are shorter in maturity than term loans, leading potentially to lower credit spreads, family firms might experience a lower cost of raising bank funding not due to reasons related to the presence of the family, but because of a change in the loan-type structure towards credit-line-type facilities in the post-crisis period. The results in Table 3.23 alleviate these concerns.

3.3.4.3 Matching Results

If firms were randomly assigned as family and non-family firms in the pre-crisis period, then it would be sufficient to make causal inferences by just comparing the outcomes of family (treated) and non-family (control) firms. However, some pre-crisis characteristics differ across the family and non-family group of firms. These differences raise the potential concern that the observed differences in firm characteristics at least partially explain both the endogenous decision of the founding family to maintain control in the firm and the ability to secure bank financing in the crisis. By just including a battery of observable firm controls in my baseline specifications to capture the additional sources of firm heterogeneity may not alleviate the potential concerns.

To assess whether pre-crisis differences between family and non family firms influence my results, I allow for nonlinear and nonparametric methods with the use of matching estimators. The rationale of using a matching estimator approach is to achieve the optimal matching of treated firms with control firms based on multiple observable characteristics, so as to restrict the set of counterfactuals to the matched controls; in other words, I expect the treated firms to behave similarly to the control group in the absence of the treatment. To define a counterfactual control group, I take advantage of the Abadie and Imbens (2011) matching estimator that minimizes the Mahalanobis distance between the set of observable firm variables prior to the exogenous shock that have been used as controls across family and non-family firms. Compared to a propensity-score-matching analysis, the Abadie-Imbens estimator provides the opportunity to achieve exact matching on categorical variables and, thus I am able to identify a control group of firms that match precisely on industry. Moreover, the process minimizes the distance among a vector of continuous covariates, including firm size, profitability, and exposure to the collapse of Lehman Brothers among others, by applying a bias-correction component to the estimates.[6]

[6]The matching process leads to similar results when cash-flow volatility is included in the covariates.

Panel A of Table 3.9 reports that following the matching process, both the family and non-family group are identical on the set of observable characteristics. Indeed, contrary to the pre-crisis differences in the univariate approach, the matching process leads to a sample with no statistically significant differences in the pre-crisis characteristics.

Having identified a matched sample of control firms, in the second stage, I compare changes in the outcome variables between the groups around the liquidity shock instead of comparing levels of the outcome variables in the treatment and control groups. Therefore, inferences about the heterogeneous exposure to the shock are based on the average effect of the treatment on the treated (ATT). The intuition behind deciding to compare differences rather than levels is that the outcome levels for treated and controls potentially differ prior to the shock, and continue being different after as well, in which case the uncontrolled firm-specific differences might bias the inferences. The outcome variables are presented in Panel B, confirming that even after I match for firms with similar observable characteristics, firms that are family-controlled experience favorable access in the syndicated loan market, as evidenced by significantly lower credit spreads. In particular, both types of firms experience an increasing cost of bank capital following the collapse of Lehman Brothers, consistent with the anecdotal evidence of limited access to the syndicated lending market; however, family-controlled firms that access the syndicated loan market obtain a lower credit spread by 26 basis points. Furthermore, when I examine loan amounts, I find that family firms have also accessed a larger pool of funds during the crisis period.

The findings of the matching process alleviate the concern that pre-crisis differences in firm characteristics may explain the impact of family control on lending terms in the crisis period and further support my results.

3.3.4.4 Changes in Credit Quality of Borrowers

In my empirical results I analyze the loan characteristics at the origination of the lending facility, thus the variables used as controls to capture the credit quality of the firms are the ones observed one time period before the origination of the lending facility. One potential concern is whether the credit quality of the borrowers changes from the pre-Lehman to the post-Lehman period differentially for family and non-family firms. For example, if family-controlled firms deleveraged (or did not increase leverage) over the pre-crisis period at a faster rate than non-family firms, the observed advantage of family-controlled firms in obtaining beneficial credit spreads is potentially explained by a differential change in credit quality.

To begin with, I have already provided evidence on the distribution of loan originations per year between family-controlled and non-family firms along with evidence that family-controlled and non-family firms did not demonstrate heterogeneity in their corporate policies. Nevertheless, in order to further alleviate any concerns, I track the credit quality variables used over time from the pre-Lehman to the post-Lehman period, and provide graphical evidence that leverage, interest expense, and credit rating between family-controlled and non-family firms follow a parallel path. Specifically, it is apparent from Figure 3.4 that in the pre-crisis period leverage ratios are relatively parallel, and, if anything, family-controlled firms leverage at a faster rate in the peak of the crisis, while Figure 3.5 and Figure 3.6 demonstrate that interest expense and credit ratings exhibit parallel paths for family-controlled and non-family firms. Moreover, it is important to mention that including in my regressions controls for changes in the credit quality variables from the pre-crisis to the crisis period, have no impact on my results.

3.3.4.5 Selection Concerns

Because the focus of my essay is on the impact of family control on loan pricing in the intensive margin of firms following the liquidity crisis, a potential concern is that family-controlled firms that accessed bank lending in the crisis period are selected from a different part of the distribution compared to non-family firms that accessed bank lending in the crisis period, and specifically that family firms that accessed bank debt during the crisis were selected from the top part of the distribution. In other words, the potential selection issue reflects the fact that the distribution of family firms rationed out is different from the distribution of non-family firms, providing room for an explanation that only a handful of the top family firms are allowed to access the bank market, while a wider part of the non-family firms distribution borrows in the crisis period, thus leading to heterogeneous loan pricing. To mitigate the potential selection issue, I compare both the firm and the loan characteristics of family and non-family firms that accessed the debt market in the pre-crisis period, but did not access the market in the crisis period. Panel A of Table 3.10 focuses on firm characteristics, whereas Panel B compares the characteristics of loans taken out by family and non-family firms. On average, family firms that got a loan in the pre-crisis period but not in the crisis period are smaller in assets than non-family firms that got a loan in the pre-crisis period but not in the crisis period (as Table 3.1 shows family firms are on average smaller in my overall sample as well). Furthermore, family firms have higher cash flow, lower leverage, and, hold more cash, though the differences are statistically insignificant. Regarding the loan characteristics, both credit spread and maturity are not significantly different between family and non-family firms. Thus the evidence in Table 3.10 provide support that selection does not drive the results.

3.3.4.6 Alternative Measure of Exposure to the Shock

In Table 3.22, I verify the validity of my results by using as an alternative measure of exposure to the liquidity shock the change in the lending supply of financial institutions following the Lehman collapse. To get an expected sign on the liquidity-shock measure that is compatible to my previous specification, I slightly divert from the construction of the measure as originally proposed in Chodorow-Reich (2014), by estimating the inverted ratio of the measure. As a result, the higher the value of the credit-supply measure, the larger the contraction in the lending activity occurring by the respective financial institution in the syndicated loan market. Table 3.22 reports the results of splitting the sample into highly exposed firms and firms experiencing limited exposure to the shock, and confirms that family-controlled firms have favorable access to the bank lending market during the crisis. Specifically, family firms are associated with a 60 basis points lower spread for highly exposed firms, which increases to 85 basis points in the case of term loans. In the credit-lines subsample, the coefficient is still negative, though statistically insignificant. Thus, my results remain robust when I use an alternative measure of exposure to the shock.

3.3.5 Robustness to Alternative Explanations

In this section, I consider several alternative hypotheses for the lower cost of obtaining access to bank financing for family firms. First, family firms might maintain a longer and tighter relation with financial institutions before the shock, and thus, the difference in the length of the lending relations between family and non-family firms rather than the family control itself might be driving the result. Furthermore, another alternative explanation that may be potentially responsible for my results is that lenders do not value family control per se, but rather concentrated blockholding in general in the presence of liquidity constraints, thus leading to a lower cost of bank capital. The intuition is that a significant ownership stake accompanies family control, and as a result, the observed difference in credit spreads is a manifestation of the different ownership structure in terms of concentrated ownership. Moreover, an additional concern is that my results are not capturing the effect of the family, but are driven by founder-run firms, which might not be family firms. Finally, I also examine whether family-controlled firms experience a lower cost in accessing bank financing, because they accept stricter covenants in the lending agreements. In the sections below, I provide additional tests to investigate and rule out these alternative channels. [7]

3.3.5.1 Length of Relationship

Lending relationships matter for the cost of borrowing (Petersen and Rajan (1994), Bharath, Dahiya, Saunders, and Srinivasan (2009)), and I next investigate whether family firms having

[7] In unreported tests, I also test whether the effect is due to differences in cash-flow volatility between family and non-family firms. I do not find any differences in the cash-flow volatility in the pre-crisis period between family and non-family firms. Furthermore, my results remain unchanged when I control for cash-flow volatility in my specifications.

longer and tighter relationships with financial institutions explains my results. In Table 3.11, I re-run my baseline specifications by controlling for measures that capture the level of lending relationships. I consider the following three measures: (1) a dummy variable equal to 1 if the firm has obtained a lending facility over the previous year from the financial institution responsible for the current lending facility, following Santos (2010); (2) the duration of the lending relationship, captured by the time elapsed since the origination of the first loan with the lender; and (3) the fraction of the syndicated loans of a firm in which a specific lender has participated. Although the measures are coarse because of limitations in the available information in the data set, considering different aspects of the lending relationship contributes to alleviating my concerns. The results are presented in Table 3.11 and demonstrate that the inclusion of controls capturing the lending relationship of a bank-firm pair has no material impact on my results.

3.3.5.2 Blockholding

Previous research has shown large blockholders affect firm valuation (i.e., Vishny and Shleifer (1986), Claessens, Djankov, Fan, and Lang (2002), Lemmon and Lins (2003)). In Table 3.12, I examine whether concentrated ownership instead of family involvement explains the results. I investigate the concentrated ownership channel by re-running my baseline specifications by separating firms based on the presence of a large shareholder instead of founding-family control. I measure concentrated ownership in two ways. The first one uses a dummy variable that takes the value of 1 if an institutional investor holds 10% or more of the common stocks of the firm. The data come from the Thompson-Reuters Institutional Holdings (13F) database that tracks the portfolios of institutional investors that are obliged to report their common stock holdings and transactions on Form 13F filed with the SEC. The second approach is to add to the presence of a large institutional shareholder with concentrated ownership the dimension of long-term orientation. The intuition is that concentrated ownership is not necessarily accompanied by active involvement in the management of the firm or with a long-term horizon perspective; thus, taking into account institutional blockholders with a long trading horizon provides a closer comparison group both in terms of ownership concentration and in temrs of horizon. The measure of investor horizon I employ is a firm-level measure used in Cella, Ellul, and Giannetti (2013) and proxies portfolio turnover by institutional investors. I classify firms with an investor-horizon value in the lowest tercile as held by long-term investors and those with an investor-horizon value in the highest tercile as held by short-term investors.

The results are presented in Table 3.12, demonstrating the presence of a large shareholder (institutional investor in my case) either as a passive investor or with a long-term horizon has no significant impact on a firm's ability to obtain bank funding under favorable terms during the crisis. Panel A reports the coefficient of the interaction term when only the presence of a large institutional shareholder with ownership greater than 10% is considered, whereas Panel B reports the coefficient of the interaction term when considering long-orientation as well. The results in Table 3.12 provide further support that creditors value family involvement and not just the presence of a large shareholder.

To provide even more compelling evidence in favor of an explanation consistent with family control being differentially valued by creditors during the crisis, I repeat the matching process of section 3.3.4.3, but now I also restrict the matching firm to be a non-family firm that not only matches in log assets, cash flow, leverage, cash, and Lehman exposure, but also has a similar level of blockholder ownership.

The results of the matching process are presented in Table 3.13, confirming that even after I match for firms with similar observable characteristics and blockholder ownership level, firms that are family-controlled experience favorable access in the syndicated loan market, as evidenced by significantly lower credit spreads. In particular, both types of firms experience an increasing cost of bank capital following the collapse of Lehman Brothers, consistent with anecdotal evidence of limited access to the syndicated lending market; however, family-controlled firms that access the syndicated loan market obtain a lower credit spread by 37 basis points.

3.3.5.3 Alternative Definition of Family Firms

In Table 3.14, I show the robustness of my results if I impose additional restrictions in the definition of family firms. Specifically, I consider three alternative definitions of family firms. The first definition requires the family to be the largest voteholder. The second definition requires the family to be the largest shareholder. Finally, the third definition requires one or more family members from the second or later generations to be officers, directors, or stockholders. My later test aims to alleviate concerns that founder-run firms that might not be family firms drive my results. Table 3.14 reports the coefficient of $Post \times I_i^{Family}$ for the different definitions of a family firm. The results remain robust.

3.3.5.4 Covenant Strictness

The results demonstrate that family-controlled firms appear to access bank financing at a lower cost compared to non-family firms. However, a potential alternative explanation is that family-controlled firms experience a lower cost in accessing bank financing, because they accept stricter covenants in the lending agreements. Therefore, the potential concern is whether family-controlled firms potentially trade off the lower funding cost for tighter covenant terms. To address the concern, I investigate whether family firms differ with respect to covenant strictness compared to non-family firms. In Table 3.24, I re-run my main specification with covenant strictness as a dependent variable. Following Murfin (2012), I construct a covenant-strictness measure that reflects the probability that a firm violates any of the covenants over the next quarter. Column (1) presents the results for the subsample of firms that were highly exposed to the Lehman collapse, whereas Column (2) focuses on firms that were less affected. In both subsamples, covenant strictness is not significantly different between family and non-family firms; thus, differences in covenant strictness do not seem to drive the wedge in credit spreads.

3.3.6 Factors Influencing the Link between Family Control and Cost of Bank Debt

The results of the main analysis show family firms have a lower cost of accessing private debt markets during the crisis. What is different about family firms? In this section, I examine potential mechanisms that can contribute to the lower cost of debt financing for family firms. First, I investigate whether the lower debt cost is concentrated in family firms expected ex ante to have larger agency costs. Second, I investigate whether the identity of the CEO matters. Finally, I explore the role of covenants.

3.3.6.1 Firm Differences in Agency Conflicts

To further assess the interpretation of my results, in Table 3.15, I repeat my analysis for firms with different ex-ante agency conflicts. If family control mitigates agency costs of debt, I would expect the impact of family control on the cost of debt during the crisis to be more pronounced in firms with higher ex-ante debt-agency conflicts. I classify firms as having a high potential for agency conflicts if they have leverage that is in the top 30% of the distribution (Panel A) or they are closer to bankruptcy, as predicted by an Altman Z-score in the top 30% of the distribution (Panel B). The results in Table 3.15 show, as before, that the impact of family control on the cost of debt is concentrated among firms with higher exposure to the shock, through their banks. Moreover, the results show that within firms with high exposure to the shock, the impact is concentrated on firms with a higher potential of agency conflicts. These results further reinforce the interpretation that family control mitigates agency conflicts between shareholders and debtholders.

3.3.6.2 Family or Outside CEO

Anderson and Reeb (2003) has shown that having a family member as CEO is associated with higher cost of debt, in the case of public debt. In Table 3.16, I investigate the relationship between family CEOs and the cost of bank debt for the family firms in my sample. CEO is a dummy variable that takes the value of 1 if the CEO is a family member, and 0 otherwise. Column (1) looks at all credit agreements, whereas Columns (2) and (3) look at term loans and credit lines, respectively. I find that in the case of private debt agreements, having a family CEO is associated with a cost of debt financing that is 49-71 basis points lower during the crisis. The coefficient of $Post \times I_i^{CEO}$ is negative and statistically significant in all three columns.

The results in Table 3.16 show the relationship between family CEO and the cost of bank debt is opposite to the one that prior literature has shown for public debt. One potential explanation comes from the fact that private debt is relationship-based. The results show that for the more relationship-based bank debt, the providers of bank capital value the fact that the family that owns a large part of the company is also the one that manages the company.

3.3.6.3 Violation of Covenants

In Table 3.17, I investigate whether family firms differ from non-family firms with respect to covenant violations. Following Nini, Smith, and Sufi (2009), I use a text-search program to collect information from 10-K and 10-Q filings on whether firms violate a covenant. I collect data on covenant violations for all firms in my sample for the years 2004-2010.

The dependent variable in Table 3.17 is an indicator that takes the value of 1 if a covenant has been violated, and 0 otherwise. In Column (1), the coefficient on I_i^{Family} shows that throughout the period 2004-2010, family firms did not have a higher propensity than non-family firms to violate their covenants. Furthermore, in Column (2), I examine whether covenant violations were different in the crisis period for family firms, but I do not find any difference between family and non-family firms. Thus, differences in covenant violations do not seem to drive the wedge in credit spreads.

3.3.6.4 Covenants on Retaining Family Involvement

Creditors use covenants to mitigate shareholder-debtholder conflicts. I provide novel evidence that in the case of family firms, creditors often use covenants that require the founding family to maintain its presence in the firm by requiring a minimum percentage of ownership or voting power. The control covenant contained in the 2009 10-K for Ralph Lauren Corporation is a typical example: "Additionally, the Credit Facility provides that an Event of Default will occur if Mr. Ralph Lauren, my Chairman of the Board and Chief Executive Officer, and entities controlled by the Lauren family fail to maintain a specified minimum percentage of the voting power of my common stock." These types of covenants show creditors value the involvement of the founding family.

In this section, I examine how common it is for creditors to require a minimum percentage of family ownership and control. For the family firms in my sample, I collect information on ownership and control covenants from the 10-K filings. Table 3.18 shows that 17% of the family firms in my sample mention the existence of control covenants in their 10-K filings at some point between 2004 and 2010. The percentage is similar when I look at family firms in which the family has more than 20% voting power or when the CEO is a member of the founding family. Across size categories, restrictions are more common in small firms, but a substantial fraction of firms with over $1 billion in book assets also have control and ownership restrictions. Because companies are not obliged to mention such agreements in their 10-K filings, Table 3.18 underestimates the usage of such restrictions.

3.4 Conclusion

The purpose of the essay is to examine the impact of family control on the agency cost of debt. In particular, I focus on the market for private debt and I analyze the potential heterogeneity in the terms of debt contracts between family and non-family firms. Using the recent financial crisis as a laboratory, I find family firms received lower rates compared to non-family firms when accessing the syndicated loan market during the crisis. Furthermore, I find tighter liquidity constraints amplify the effect of family control on the cost of debt.

To alleviate any potential concerns (e.g., due to family firms matching with better financial institutions), I exploit the cross-sectional dispersion in lender health induced by the collapse of Lehman Brothers as a source of exogenous variation in the exposure to the shock. Specifically, I hypothesize that if financial frictions make it difficult for firms to access external capital or switch from one source of capital to another, firms that maintained lending relationships with financial institutions that were highly exposed to the negative liquidity shock during the crisis experienced tighter financial constraints and a higher cost of accessing the syndicated loan market. Indeed, the government decision not to provide financial support and to allow Lehman Brothers to go bankrupt was considered unexpected, thus providing a useful laboratory to identify the relationship between private debt markets and family control. Therefore, the research design that I consider compares family and non-family firms at the same point in time that have been subject to the same shock and have similar exposure to the shock based on their lending relationships with differentially liquidity-struck financial institutions. Thus I am able to tease out the effect of family control on the cost of debt.

Furthermore, I unveil potential channels that drive the relation between family control and loan spreads. I find the gap in spreads between family and non-family firms is higher in firms with higher ex-ante shareholder-debtholder agency conflicts. Moreover, the effect is stronger when family CEOs run the firms. Finally, I investigate the importance of covenants linked with founding-family control as a manifestation of the importance of family control for lending relations and the role of long-term orientation on the cost of accessing the private debt market.

3.5 Figures and Tables

3.5.1 Figures

Figure 3.1: Credit Spread

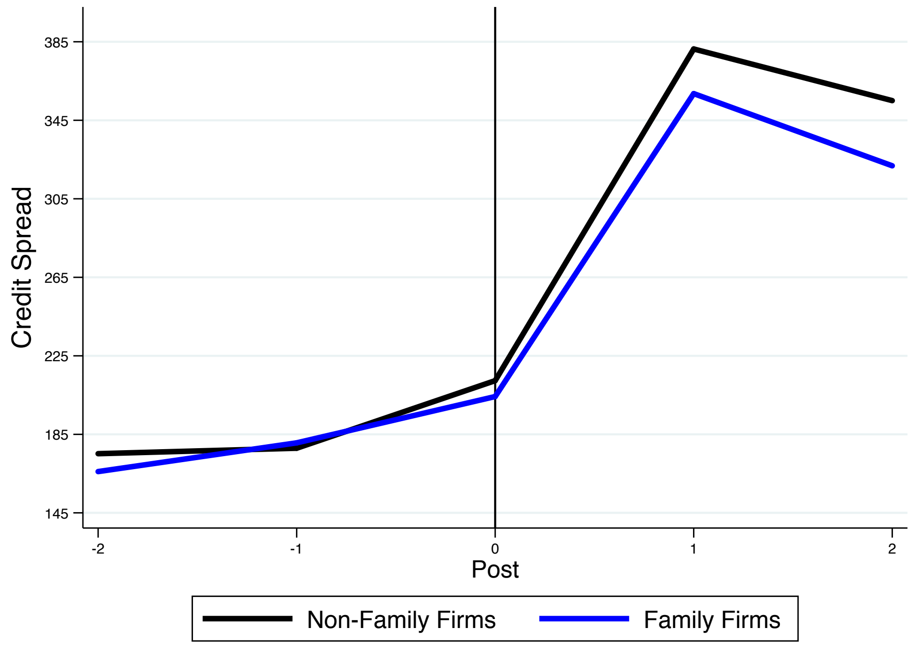


Figure 3.2: Credit Spread - High Lehman Exposure

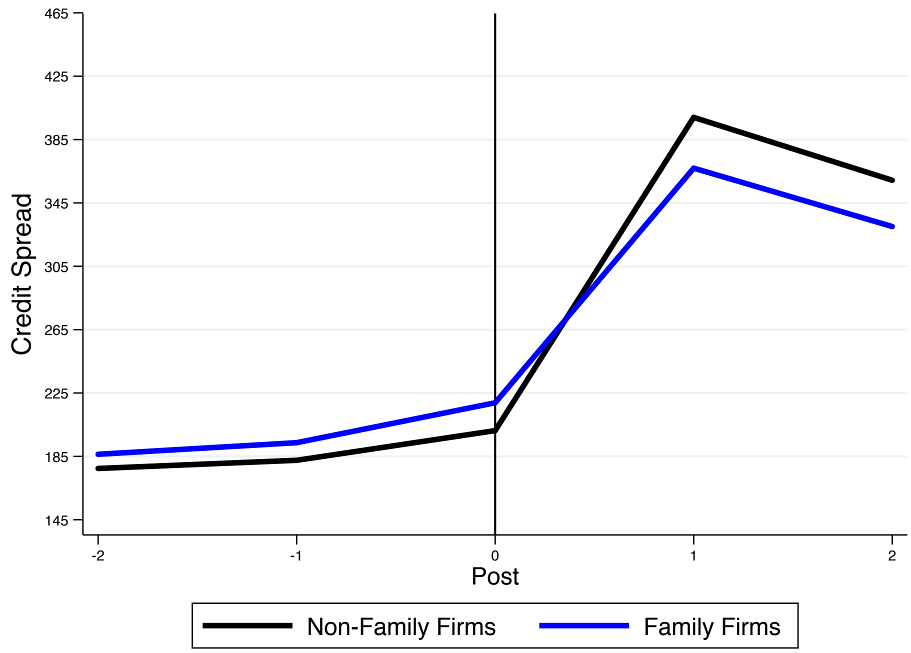


Figure 3.3: Credit Spread - Low Lehman Exposure

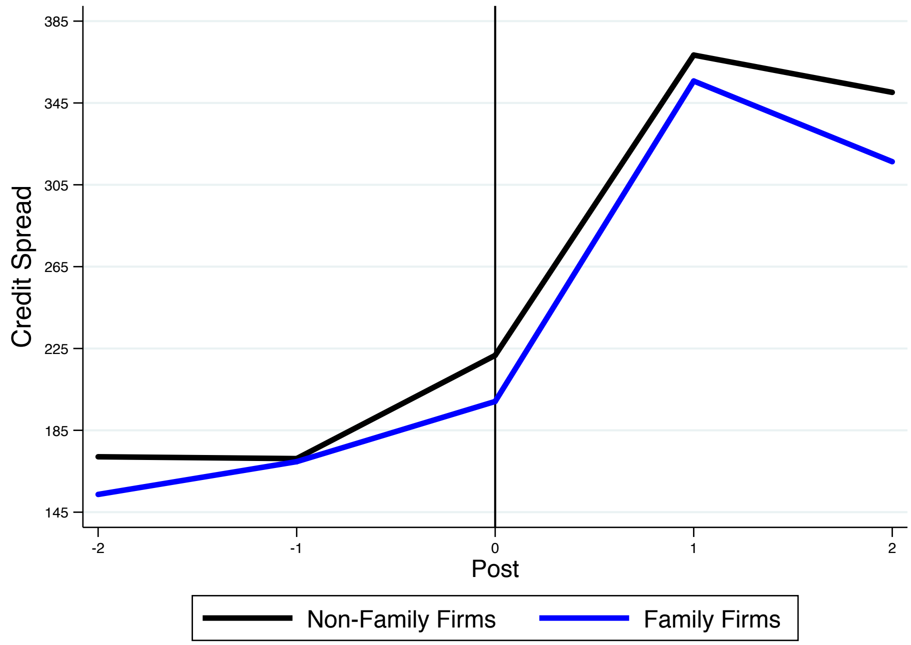


Figure 3.4: Leverage

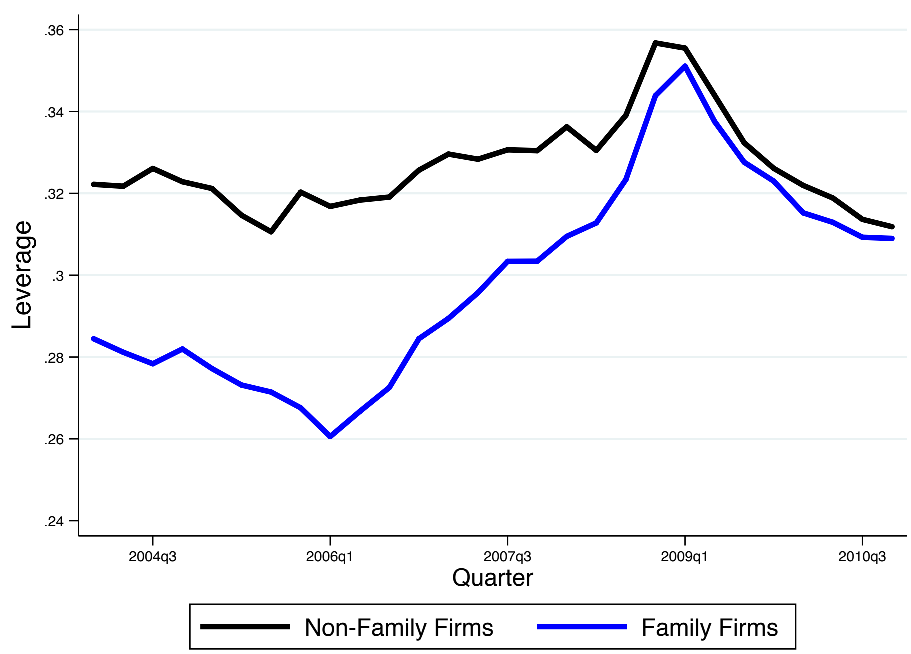


Figure 3.5: Interest Expense

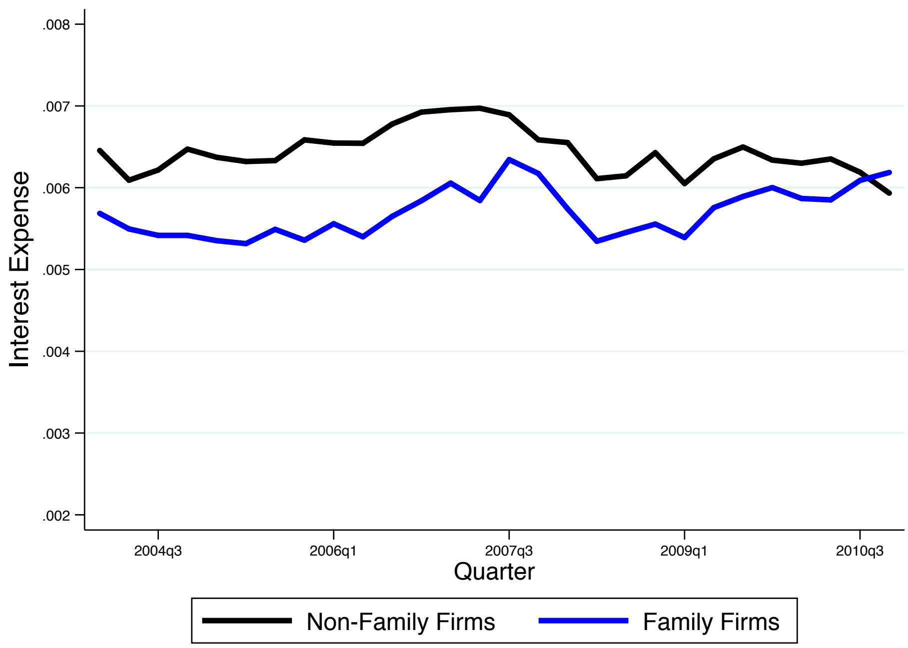
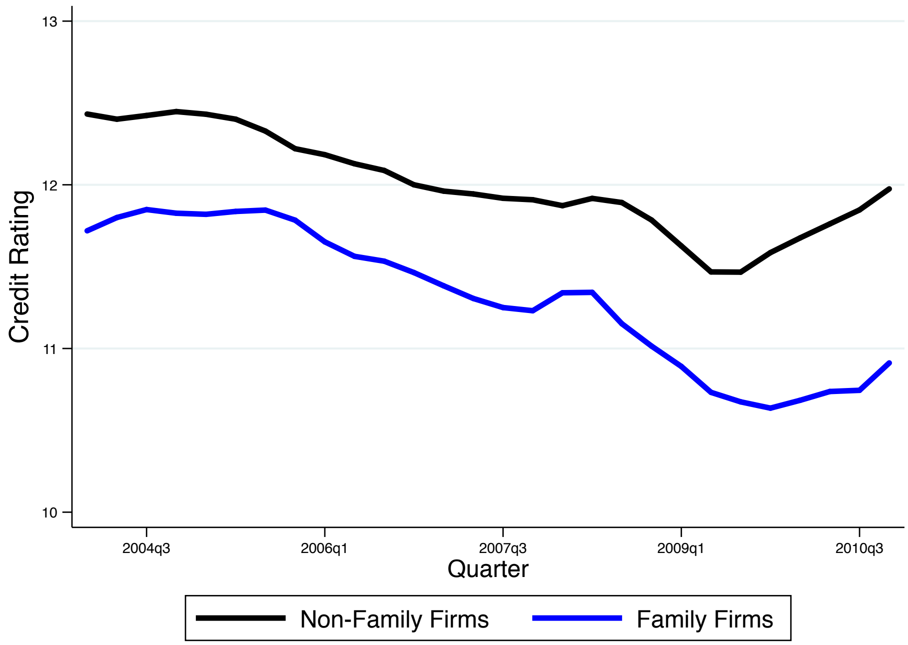


Figure 3.6: Credit Rating



3.5.2 Tables

Table 3.1: Summary Statistics

	Pre-Crisis												
	Panel A: Firm Variables												
	Family Firms						**Non-Family Firms**						
Variables	**N**	**Mean**	**Std Dev.**	**p10**	**p50**	**p90**	**N**	**Mean**	**Std Dev.**	**p10**	**p50**	**p90**	**Difference**
Log Assets	1,103	7.17	1.64	5.08	7.05	9.43	2,937	7.65	1.67	5.59	7.56	9.97	-0.47***
Cash Flow	1,080	0.037	0.031	0.012	0.036	0.069	2,893	0.035	0.035	0.012	0.033	0.064	0.002
Leverage	1,103	0.34	0.25	0.04	0.33	0.64	2,937	0.38	0.28	0.08	0.32	0.70	-0.03
Interest Expense	981	0.007	0.009	0.001	0.005	0.014	2,703	0.008	0.008	0.001	0.005	0.016	0
Cash	1,103	0.066	0.102	0.003	0.031	0.155	2,937	0.065	0.090	0.003	0.033	0.158	0.001
S&P Rating	610	12	4	8	11	17	2,000	12	4	8	11	17	0
Age	1,116	17	14	3	13	42	2,945	25	19	5	18	56	-8***
Number of Firms	313						858						
	Panel B: Loan Variables												
	Family Firms						**Non-Family Firms**						
Variables	**N**	**Mean**	**Std Dev.**	**p10**	**p50**	**p90**	**N**	**Mean**	**Std Dev.**	**p10**	**p50**	**p90**	**Difference**
Credit Spread													
All Loans	1,149	190	137	50	175	325	3,232	187	142	35	175	350	2.76
Term Loans	386	254	159	113	225	450	1,081	267	162	100	225	475	-13.70
Credit Lines	763	157	111	33	150	275	2,151	146	111	30	125	288	10.82
Maturity	1,117	54	21	22	60	81	3,187	53	21	12	60	81	0.55
Log Amount	1,149	19.06	1.42	17.22	19.11	20.91	3,232	19.24	1.36	17.50	19.28	20.95	-0.17
Lehman Exposure	1,149	0.071	0.054	0.037	0.058	0.119	3,232	0.074	0.047	0.041	0.065	0.116	-0.003

	Panel C: Bank Variables					
Variables	**N**	**Mean**	**Std Dev.**	**p10**	**p50**	**p90**
Assets (in million $)	71	550.2	622.2	48.4	206.9	1,494.1
Deposits/Assets	71	0.24	0.13	0.12	0.23	0.40
Profitability	71	0.006	0.006	0.002	0.006	0.013
Capital/Assets	71	0.079	0.032	0.034	0.085	0.105
Loan Net Charge-Offs	71	0.002	0.002	0	0.001	0.005
Lehman Exposure	71	0.068	0.082	0.013	0.057	0.119
Number of Loans Ratio (Post/Pre)	71	0.50	0.56	0.10	0.38	0.89

Notes: The table reports descriptive statistics for the sample used in the analysis, broken down by family and non-family firms. Panel A provides summary statistics on firm characteristics in the period before the crisis. The last column in Panel A provides the difference in means between the characteristics of family and non-family firms. Panel B describes the characteristics of loans taken out by firms in the pre-crisis period. The last column in Panel B provides the difference in means between the characteristics of the loans taken out by family and non-family firms. Panel C provides summary statistics on the bank characteristics for the pre-crisis period.

Table 3.2: Loan Origination Distribution

Distribution of Loan Facilities Per Origination Year						
	Family Firms			Non-Family Firms		
Year	**N**	**# of Loans**	**% of Loans**	**N**	**# of Loans**	**% of Loans**
2004	170	325	18%	471	855	17%
2005	157	298	17%	497	910	18%
2006	160	274	16%	454	836	17%
2007	126	222	13%	353	669	13%
2008	113	176	10%	290	459	9%
2009	128	193	11%	355	527	10%
2010	188	272	15%	495	784	16%
Total		1,760			5,040	

Notes: The table reports the distribution of loan facilities per origination year broken down by family and non-family firms.

Table 3.3: Industry Distribution

Industry	Non-Family Firms N	Non-Family Firms Percentage	Family Firms N	Family Firms Percentage	Total	Family Firms in Industry
Agriculture	6	0.7%	1	0.3%	7	14%
Aircraft	5	0.6%	3	1.0%	8	38%
Apparel	7	0.8%	7	2.2%	14	50%
Automobiles & Trucks	23	2.7%	6	1.9%	29	21%
Beer & Liquor	1	0.1%	1	0.3%	2	50%
Business Services	71	8.3%	38	12.1%	109	35%
Business Supplies	14	1.6%	8	2.6%	22	36%
Candy & Soda	2	0.2%	2	0.6%	4	50%
Chemicals	33	3.9%	6	1.9%	39	15%
Coal	9	1.1%	1	0.3%	10	10%
Computers	11	1.3%	7	2.2%	18	39%
Construction	10	1.2%	6	1.9%	16	38%
Construction Materials	25	2.9%	3	1.0%	28	11%
Consumer Goods	19	2.2%	6	1.9%	25	24%
Defense	3	0.4%	0	0.0%	3	0%
Electrical Equipment	6	0.7%	1	0.3%	7	14%
Electronic Equipment	30	3.5%	10	3.2%	40	25%
Entertainment	13	1.5%	6	1.9%	19	32%
Fabricated Products	1	0.1%	0	0.0%	1	0%
Food Products	23	2.7%	10	3.2%	33	30%
Gold	3	0.3%	0	0.0%	3	0%
Healthcare	26	3.0%	7	2.2%	33	21%
Machinery	28	3.3%	9	2.9%	37	24%
Measuring & Control Equipment	9	1.1%	2	0.6%	11	18%
Medical Equipment	14	1.6%	6	1.9%	20	30%
Metals & Mining	5	0.6%	0	0.0%	5	0%
Other	3	0.4%	1	0.3%	4	25%
Personal Services	10	1.2%	8	2.6%	18	44%
Petroleum & Natural Gas	54	6.3%	36	11.5%	90	40%
Pharmaceutical Products	17	2.0%	3	1.0%	20	15%
Printing & Publishing	13	1.5%	9	2.9%	22	41%
Recreation	5	0.6%	2	0.6%	7	29%
Restaurants & Hotels	31	3.6%	14	4.5%	45	31%
Retail	68	7.9%	29	9.3%	97	30%
Rubber & Plastic Products	13	1.5%	2	0.6%	13	13%
Shipbuilding & Railroad Equipment	3	0.4%	0	0.0%	3	0%
Shipping Containers	6	0.7%	1	0.3%	7	14%
Steel	18	2.1%	3	1.0%	21	14%
Telecommunications	34	4.0%	28	9.0%	62	45%
Textiles	10	1.2%	1	0.3%	11	9%
Tobacco Products	1	0.1%	0	0.0%	1	0%
Transportation	25	2.9%	13	4.2%	38	34%
Utilities	109	12.7%	4	1.3%	113	4%
Wholesale	41	4.8%	13	4.2%	54	24%
Total	**858**		**313**		**1,171**	

Notes: The table presents the industry distribution of firms by family ties. Firms are sorted by industry using the Fama-French 48-industry classification.

Table 3.4: Family Control and Credit Spread

Panel A: Univariate Test			
	(1)	**(2)**	**(3)**
Variables	**Family**	**Non-Family**	**Difference**
Pre-Crisis	176.89***	171.81***	5.08
Crisis	306.82***	323.93***	-17.12*
Difference	129.93***	152.12***	
Difference-in-Differences			-22.20**

Panel B: Regression Analysis			
	(1)	**(2)**	**(3)**
Variables			
Post	170.04***	160.86***	167.74***
	(7.85)	(13.62)	(13.312)
I_i^{Family}	-0.43	10.52	
	(8.27)	(6.91)	
$Post \times I_i^{Family}$	-28.96**	-24.69**	-20.60*
	(12.46)	(11.64)	(12.22)
Firm Controls	No	Yes	Yes
Bank Controls	No	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Bank Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	No	No	Yes
Observations	6,157	4,965	4,965
Adjusted R^2	0.30	0.42	0.62

Notes: The table reports the change in loan spreads during the crisis for family and non-family firms. Panel A presents the univariate test. Column (1) presents mean loan spreads for family firms, column (2) for non-family firms, and column (3) provides the difference in means between the loan spreads for family and non-family firms. Panel B report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{it-1} + \theta Z_{bt-1} + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. I include bank controls (Z) that include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In column (3) I include firm fixed effects. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.5: Credit Spread by Lehman Exposure

	(1)	(2)	(3)	(4)
Variables				
	High Lehman		**Low Lehman**	
Post	185.83***	191.90***	136.73***	132.87***
	(30.48)	(24.17)	(19.37)	(21.64)
I_i^{Family}	23.89		-9.17	
	(15.83)		(12.24)	
$Post \times I_i^{Family}$	-75.54***	-51.52**	1.66	2.05
	(25.31)	(23.44)	(18.61)	(21.73)
Firm Controls	Yes	Yes	Yes	Yes
Bank Controls	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	No	Yes	No
Firm Fixed Effects	No	Yes	No	Yes
Observations	1,211	1,211	1,333	1,333
Adjusted R^2	0.45	0.64	0.38	0.55

Notes: The table explores the change in credit spreads during the crisis for family and non-family firms. The sample is divided into firms that maintain relationships with financial institutions highly exposed ($LehmanExposure_i$ is in the top 25% of the distribution) or less exposed (bottom 25%) to the Lehman collapse, so that I compare family and non-family firms with similar exposure to the shock. Lehman Exposure measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{it-1} + \theta Z_{bt-1} + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a that variable which takes the value 1 for family firms, and 0 for non-family firms. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. I include controls for bank characteristics (Z_{bt-1}). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In specifications (2) and (4), I add firm fixed effects. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6: Other Contract Terms

	(1)	(2)	(3)	(4)
Variables				
	Maturity		**Loan Amount**	
$Post$	-10.44***	-12.29***	-0.07	-0.29***
	(0.80)	(1.43)	(0.05)	(0.10)
I_i^{Family}	1.14	1.77	-0.04	0.02
	(1.24)	(1.17)	(0.09)	(0.06)
$Post \times I_i^{Family}$	-0.07	-0.54	0.09	0.12*
	(1.35)	(1.38)	(0.08)	(0.07)
Firm Controls	No	Yes	No	Yes
Bank Controls	No	Yes	No	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	6,608	5,357	6,777	5,463
Adjusted R^2	0.12	0.18	0.17	0.56

Notes: The table explores the change in other contract terms during the crisis for family and non-family firms. I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{it-1} + \theta Z_{bt-1} + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is either the maturity or the log amount on loans, i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable which takes the value 1 for family firms and 0 for non-family firms. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense and S&P rating. Z are bank controls. Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects, and η_b bank fixed effects. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.7: Extensive Margin

	(1)	**(2)**	**(3)**
Variables			
$LehmanExposure$	0.89		1.01
	(0.76)		(0.86)
I_i^{Family}		0.06	0.08
		(0.06)	(0.12)
$I_i^{Family} \times LehmanExposure$			-0.24
			(1.72)
Firm Controls	Yes	Yes	Yes
Bank Controls	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes
Observations	3,492	3,492	3,492
Adjusted R^2	0.32	0.31	0.48

Notes: The table reports the results of the regressions that consider the impact of exposure to the collapse of Lehman and family control on the likelihood of obtaining bank credit during the crisis. The sample contains all the borrowers that obtained a syndicated loan in the pre-crisis period. The dependent variable is an indicator corresponding to whether the borrower who had obtained a loan in the pre-crisis period obtained a new loan commitment between October 2008 and December 2010. $LehmanExposure$ measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. I include controls for bank characteristics. Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.8: Financing Decisions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
	High Lehman				**Low Lehman**			
	Dividends	**Cash**	**Leverage**	**S-T Debt**	**Dividends**	**Cash**	**Leverage**	**S-T Debt**
I_i^{Family}	-0.001	0.005	0.042	0.001	0.001	0.008	-0.019	-0.025
	(0.001)	(0.013)	(0.030)	(0.020)	(0.001)	(0.013)	(0.021)	(0.032)
$Post \times I_i^{Family}$	0.000	0.002	-0.011	0.024	0.001	-0.010	0.024	0.006
	(0.001)	(0.011)	(0.020)	(0.024)	(0.001)	(0.010)	(0.016)	(0.035)
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,167	6,384	6,189	5,952	7,090	7,384	7,120	6,231
Adjusted R^2	0.03	0.34	0.54	0.18	0.04	0.31	0.42	0.21

Notes: The table reports the change in financing decisions during the crisis for family and non-family firms. The sample is divided into firms that maintain relationships with financial institutions highly exposed ($LehmanExposure_i$ is in the top 25% of the distribution) or less exposed (bottom 25%) to the Lehman collapse. Lehman Exposure measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.9: ATT Matching Estimator

Panel A: Control Variables			
	(1)	**(2)**	**(3)**
Variables	**Family**	**Non-Family**	**Difference**
Log Assets	6.75***	6.74***	0.01
Cash Flow	0.036***	0.036***	0.000
Leverage	0.28***	0.30***	-0.02
Cash	0.079***	0.084***	-0.004
Lehman Exposure	0.061***	0.060***	0.001

Panel B: Outcomes			
	(1)	**(2)**	**(3)**
Variables	**Family**	**Non-family**	**Difference**
		Credit Spread	
Before	176.89***	171.81***	5.08
After	306.82***	323.93***	-17.12*
Difference	129.93***	152.12***	
Difference-in-Differences			-22.20**
Matching Estimator			-26.01***
		Maturity	
Before	52.79***	51.78***	1.01
After	43.05***	41.81***	1.24
Difference	-9.74***	-9.97***	
Difference-in-Differences			0.23
Matching Estimator			-0.85
		Loan Amount	
Before	18.81***	19.07***	-0.25***
After	18.96***	19.07***	-0.11
Difference	0.15***	0.01	
Difference-in-Differences			0.14**
Matching Estimator			0.09

Notes: The table presents the results of the difference-in-differences Abadie and Imbens matching estimator around the Lehman collapse, imposing treated and control firms to be similar in terms of observable characteristics. Treated firms are the family-controlled firms. Panel A reports that following the matching process, both the family and non-family group are identical on the set of observable characteristics. Panel B compares changes in the outcome variables between family and non-family firms around the liquidity shock. The dependent variables include the credit spread, the maturity, and the amount of the lending facilities. Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.10: Robustness to Selection

Pre-Crisis													
Panel A: Firm Variables													
	Family Firms						**Non-Family Firms**						
Variables	**N**	**Mean**	**Std Dev.**	**p10**	**p50**	**p90**	**N**	**Mean**	**Std Dev.**	**p10**	**p50**	**p90**	**Difference**
Log Assets	1,049	6.57	1.63	4.45	6.57	8.66	2,721	7.24	1.93	4.63	7.31	9.83	-0.67***
Cash Flow	995	0.036	0.045	-0.001	0.036	0.077	2,562	0.035	0.036	0.006	0.033	0.069	0.002
Leverage	1,049	0.31	0.23	0.04	0.28	0.60	2,721	0.33	0.22	0.07	0.31	0.61	-0.03
Interest Expense	952	0.007	0.008	0.001	0.005	0.014	2,483	0.007	0.007	0.001	0.005	0.013	0
Cash	1,047	0.097	0.124	0.006	0.048	0.249	2,721	0.086	0.115	0.005	0.039	0.218	0.011
S&P Rating	433	12	3	8	11	15	1,588	12	4	8	12	17	0
Number of Firms	384						831						
Panel B: Loan Variables													
	Family Firms						**Non-Family Firms**						
Variables	**N**	**Mean**	**Std Dev.**	**p10**	**p50**	**p90**	**N**	**Mean**	**Std Dev.**	**p10**	**p50**	**p90**	**Difference**
Credit Spread													
All Loans	955	196	144	50	175	350	2,478	187	141	40	175	350	9
Term Loans	270	280	177	113	225	565	769	264	161	100	225	450	17
Credit Lines	685	163	112	45	150	300	1,709	152	116	30	125	300	11
Maturity													
All Loans	1,023	51	20	18	60	72	2,650	52	22	12	60	78	-1
Term Loans	289	60	22	35	50	84	813	60	23	26	60	84	0
Credit Lines	734	47	19	13	59	60	1,836	48	20	12	60	60	-1

Notes: The table reports descriptive statistics for the sample of firms that accessed the syndicated loan market in the pre-crisis period but not in the crisis period. The summary statistics are decomposed into family and non-family firms. Panel A provides summary statistics on firm characteristics in the period before the crisis. The last column in Panel A provides the difference in means between the characteristics of family and non-family firms. Panel B describes the characteristics of loans taken out by firms. The last column in Panel B provides the difference in means between the characteristics of the loans taken out by family and non-family firms in the pre-crisis period.

Table 3.11: Lending-Relationship Control

	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
	High Lehman			**Low Lehman**		
	Last-Year Lending	**Relationship Time**	**Fraction of Lending**	**Last-Year Lending**	**Relationship Time**	**Fraction of Lending**
Post	181.46***	192.15***	174.43***	129.81***	119.80***	144.10***
	(33.20)	(36.19)	(27.59)	(19.50)	(22.78)	(26.64)
I_i^{Family}	22.88	20.34	23.29	-9.24	-7.57	-9.42
	(15.86)	(16.72)	(15.99)	(12.22)	(12.03)	(12.18)
$Post \times I_i^{Family}$	-74.79***	-71.40***	-78.38***	-2.05	0.54	1.85
	(25.32)	(25.66)	(26.84)	(18.05)	(18.72)	(18.69)
Relationship	-16.64	-0.67	12.20	0.52	-1.27***	12.20
	(11.14)	(0.41)	(20.36)	(10.37)	(0.42)	(16.69)
$Post \times Relationship$	0.41	-0.11	19.76	46.97	1.32**	-10.93
	(46.48)	(0.68)	(46.48)	(29.41)	(0.59)	(31.84)
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,211	1,211	1,211	1,333	1,333	1,333
Adjusted R^2	0.42	0.43	0.42	0.35	0.35	0.35

Notes: The table reports the change in credit spreads during the crisis for family and non-family firms. The sample is divided into firms that maintain relationships with financial institutions highly exposed ($LehmanExposure_i$ is in the top 25% of the distribution) or less exposed (bottom 25%) to the Lehman collapse, so that I compare family and non-family firms with similar exposure to the shock. Lehman Exposure measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{i,t-1} + \theta Z_{b,t-1} + \phi Relationship_{ibt} + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. $Relationship_{ibt}$ is one of the following: (1) a dummy variable equal to 1 if the firm has obtained a lending facility over the previous year from the financial institution responsible for the current lending facility following Santos (2010), (2) the duration of the lending relationship, captured by the time elapsed since the origination of the first loan with the lender, or (3) the fraction of the syndicated loans of a firm in which a specific lender has participated. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. I include controls for bank characteristics ($Z_{b,t-1}$). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.12: Institutional Ownership

	(1)	(2)
Panel A: Concentrated Ownership		
$Post \times I_i^{Concentrated}$	-1.77	2.95
	(11.37)	(10.93)
Firm Controls	Yes	Yes
Bank Controls	Yes	Yes
Bank Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	No
Firm Fixed Effects	No	Yes
Observations	5,218	5,218
Adjusted R^2	0.42	0.62
Panel B: Concentrated Ownership with Long-Term Horizon		
$Post \times I_i^{Long-Concentrated}$	14.10	13.35
	(13.16)	(13.21)
Firm Controls	Yes	Yes
Bank Controls	Yes	Yes
Bank Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	No
Firm Fixed Effects	No	Yes
Observations	2,868	2,868
Adjusted R^2	0.50	0.68

Notes: The table reports the change in credit spreads of credit loans during the crisis for firms with an institutional owner that holds 10% or more of the common stock of the firm. To account for the heterogeneous exposure to the shock, the sample is divided into firms that maintain relationships with financial institutions highly exposed ($LehmanExposure_i$ is in the top 25% of the distribution) or less exposed (bottom 25%) to the Lehman collapse. Lehman Exposure measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Concentrated} + \delta Post_{it} \times I_i^{Concentrated} + \zeta X_{i,t-1} + \theta Z_{b,t-1} + \phi Relationship_{ibt} + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. In Panel A, $I_i^{Concentrated}$ is a dummy variable that takes the value 1 for firms with an institutional investor that holds 10% or more of the common stock of the firm, and 0 otherwise, whereas in Panel B, $I_i^{Long-Concentrated}$ is a dummy variable that takes the value 1 for firms with an institutional investor that holds 10% or more of the common stock of the firm and has a long-term horizon as defined in Subsection 3.3.5.2, and 0 otherwise. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. I include controls for bank characteristics (Z). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.13: **ATT Matching Estimator with Ownership**

	(1)	(2)	(3)
Variables	**Family**	**Non-family**	**Difference**
		Credit Spread	
Before	174.91***	158.37***	16.54**
After	302.38***	307.60***	-5.22
Difference	127.46***	149.23***	
Difference-in-Differences			-21.76**
Matching Estimator			-36.86***

Notes: The table presents the results of the difference-in-differences Abadie and Imbens matching estimator around the Lehman collapse, imposing treated and control firms to be similar in terms of observable characteristics including the level of ownership. Treated firms are the family-controlled firms. The table compares changes in the credit spreads between treated and control firms around the liquidity shock. The dependent variables that have been considered are the credit spread, the maturity, and the amount of the lending facilities.
Significance Levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.14: Alternative Definitions of Family Firms

	(1)	(2)	(3)
Variables			
	Number of Family Firms	**High Lehman**	**Low Lehman**
Largest Voteholder	171	-58.04*	1.06
		(31.72)	(26.39)
Largest Shareholder	167	-58.04*	3.68
		(31.72)	(27.51)
Second Generation	145	-52.01**	23.59
		(24.39)	(18.07)

Notes: The table reports, for different definitions of a family firm, the coefficient of $Post_{it} \times I_i^{Family}$ based on the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{i,t-1} + \theta Z_{b,t-1} + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. I include controls for bank characteristics (Z). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. I consider three alternative definitions of family firms: (1) The family is the largest voteholder, (2) the family is the largest shareholder, and (3) one or more family members from the second or later generations are officers, directors, or stockholders. The sample is divided into firms that maintain relationships with financial institutions highly exposed ($LehmanExposure_i$ is at the top 25% of the distribution) or less exposed (bottom 25%) to the Lehman collapse, so that I compare family and non-family firms with similar exposure to the shock. Lehman Exposure measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.15: Firm-Level Differences in Agency Conflicts

	Panel A: Leverage			
	(1)	**(2)**	**(3)**	**(4)**
Variables				
	High Lehman		**Low Lehman**	
	High Leverage	**Low Leverage**	**High Leverage**	**Low Leverage**
Post	227.39***	237.06***	124.39***	110.12***
	(59.01)	(38.35)	(43.67)	(21.53)
I_i^{Family}	24.57	2.07	-23.95	-4.54
	(22.37)	(23.29)	(28.80)	(16.72)
$Post \times I_i^{Family}$	-102.41**	-66.29	-33.81	-12.34
	(45.60)	(44.86)	(46.81)	(22.12)
Firm and Bank Controls	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Observations	543	302	196	757
R^2	0.38	0.65	0.34	0.39

	Panel B: Altman's Z-Score			
	(1)	**(2)**	**(3)**	**(4)**
	High Lehman		**Low Lehman**	
	High Z-Score	**Low Z-Score**	**High Z-Score**	**Low Z-Score**
Post	139.81***	243.84***	165.26***	127.36***
	(26.70)	(28.10)	(30.80)	(22.18)
I_i^{Family}	37.02	37.44	10.56	-5.75
	(23.41)	(24.11)	(27.43)	(15.45)
$Post \times I_i^{Family}$	-68.24**	-46.73	-25.28	25.67
	(29.733)	(36.608)	(48.401)	(21.35)
Firm and Bank Controls	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	576	329	304	683
Adjusted R^2	0.50	0.63	0.40	0.43

Notes: The table presents the change in credit spreads during the crisis for family and non-family firms based on the heterogeneous exposure to the shock and a firm-level measure of differences in ex-ante agency conflicts. In Panel A, firms are divided based on their pre-crisis leverage, whereas in Panel B, firms are divided based on their pre-crisis Altman's Z-Score. The dependent variable is the loan credit spread. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating and I include controls for bank characteristics - assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.16: Family CEO and Credit Spread

	(1)	(2)	(3)
Variables			
	All Loans	**Term Loans**	**Credit Lines**
$Post$	151.22***	161.27***	160.20***
	(25.81)	(46.09)	(23.12)
I_i^{CEO}	14.16	28.16	16.22
	(10.98)	(24.44)	(10.47)
$Post \times I_i^{CEO}$	-54.53***	-70.78*	-49.30***
	(20.44)	(41.42)	(18.22)
Firm Controls	Yes	Yes	Yes
Bank Controls	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes
Observations	1,411	418	993
Adjusted R^2	0.50	0.50	0.54

Notes: The table explores the relationship between family CEOs and the cost of private debt for family firms. The sample is restricted to family firms. I_i^{CEO} is a dummy variable that takes the value 1 if the CEO is a family member, and 0 otherwise. Column (1) incorporates all credit agreements, whereas column (2) and (3) focus on term loans and credit lines, respectively. Firm controls include size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating, whereas bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. Furthermore, industry, and bank fixed effects are included. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.17: Covenant Violation

	(1)	(2)
Variables		
I_i^{Family}	0.024	0.010
	(0.048)	(0.056)
$Post$		-0.064
		(0.059)
$Post \times I_i^{Family}$		0.050
		(0.100)
Firm Controls	Yes	Yes
Observations	26,532	26,532
Adjusted R^2	0.09	0.13

Notes: The table reports the relationship between family control and the incidence of violating a covenant. The dependent variable is an indicator that takes the value of 1 if a covenant has been violated, and 0 otherwise. *Post* is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. Column (1) reports the impact of family control, whereas column (2) examines whether there is a change in the relation between family control and covenant violation during the crisis. Firm controls include size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.18: Control Covenants

	Fraction with Control Covenant
Fraction of Family Firms	0.17
Fraction of Family Firms With Voting Power of 20% or More	0.17
Family CEO Firms	0.18
By Size	
$<$ \$100M	0.20
\$100M - \$250M	0.13
\$250M - \$500M	0.23
\$500M - \$1000M	0.23
\$1000M - \$2,500M	0.12
\$2,500M - \$5,000M	0.15
$\geq$ \$5,000M	0.15

Notes: The table provides information on the fraction of family-controlled firms that have ownership and control covenants, as reported in the 10-K fillings.

Table 3.19: Lehman Exposure and Credit Spread

	(1)	(2)
Variables		
Post	145.04***	138.77***
	(10.13)	(13.35)
LehmanExposure	44.02	22.20
	(77.84)	(67.30)
Post × *LehmanExposure*	258.01**	222.10**
	(44.94)	(44.67)
Firm Controls	No	Yes
Bank Controls	No	Yes
Industry Fixed Effects	Yes	Yes
Bank Fixed Effects	Yes	Yes
Observations	6,141	5,151
Adjusted R^2	0.30	0.41

Notes: The table investigates the change in loan spreads during the crisis for the firms in my sample. I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma LehmanExposure_i + \delta Post_{it} \times LehmanExposure_i + \zeta X_{it-1} + \theta Z_{bt-1} + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. Lehman Exposure measures the exposure to the liquidity shock and is constructed following Ivashina and Scharfstein (2010). X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. Furthermore, I include controls for bank characteristics (Z). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.20: Credit Spread by Lehman Exposure for Term Loans and Credit Lines

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
	Term Loans				**Credit Lines**			
	High Lehman		**Low Lehman**		**High Lehman**		**Low Lehman**	
$Post$	257.96***	235.38***	152.44***	173.53***	181.14***	163.08***	122.63***	120.63***
	(33.81)	(62.37)	(28.39)	(40.77)	(10.90)	(18.26)	(9.14)	(14.57)
I_i^{Family}	-19.30	-0.98	7.45	-3.06	30.27	26.38	-23.18	-11.35
	(27.01)	(21.53)	(35.21)	(37.30)	(19.61)	(17.01)	(17.44)	(9.51)
$Post \times I_i^{Family}$	-123.10***	-120.87***	-36.17	-25.15	-42.05*	-46.95**	15.55	13.66
	(40.47)	(45.58)	(41.22)	(42.82)	(23.73)	(19.99)	(16.38)	(15.04)
Firm Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	614	471	409	359	829	740	1,043	974
Adjusted R^2	0.32	0.37	0.22	0.23	0.45	0.56	0.35	0.46

Notes: The table presents the change in credit spreads during the crisis for family and non-family firms for different loan types. The sample is divided into firms that maintain relationships with financial institutions highly exposed ($LehmanExposure_i$ is in the top 25% of the distribution) or less exposed (bottom 25%) to the Lehman collapse, so that I compare family and non-family firms with similar exposure to the shock.Lehman Exposure measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{i,t-1} + \theta Z_{b,t-1} + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. Furthermore, I include controls for bank characteristics (Z). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.21: Family Control and Credit Spread - Continuous Lehman Measure

	(1)	(2)	(3)
Variables			
	All Loans	**Term Loans**	**Credit Lines**
$Post$	141.25***	159.81***	130.45***
	(15.82)	(28.01)	(12.15)
I_i^{Family}	-16.95	-21.05	-20.13
	(11.42)	(15.57)	(12.39)
$LehmanExposure$	-23.71	-48.63	6.02
	(74.66)	(86.54)	(63.78)
$Post \times I_i^{Family}$	11.50	16.26	19.95
	(19.85)	(33.72)	(19.78)
$Post \times LehmanExposure$	359.17***	387.79**	259.30**
	(136.55)	(197.01)	(104.72)
$I_i^{Family} \times LehmanExposure$	302.92**	225.16*	341.95*
	(148.904)	(132.528)	(177.309)
$Post \times I_i^{Family} \times LehmanExposure$	-522.36**	-549.71**	-510.95**
	(233.82)	(276.89)	(251.65)
Firm Controls	Yes	Yes	Yes
Bank Controls	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes
Observations	5,075	1,507	3,568
Adjusted R^2	0.43	0.38	0.52

Notes: The table presents the change in credit spreads during the crisis for family and non-family firms for different loan types based on the heterogeneous exposure to the shock. $LehmanExposure_i$ measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma LehmanExposure_i + \theta I_i^{Family} + \mu I_i^{Family} \times Post_{it} + \nu Family_i \times LehmanExposure_i + \delta Post_{it} \times LehmanExposure_i + \phi Post_{it} \times LehmanExposure_i \times I_i^{Family} + \zeta X_{i,t-1} + \chi Z_{b,t-1} + \eta_{bi} + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. Furthermore, I include controls for bank characteristics (Z). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.22: Credit Spread - High/Low Delta

	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
	High Delta			**Low Delta**		
	All Loans	**Term Loans**	**Credit Lines**	**All Loans**	**Term Loans**	**Credit Lines**
$Post$	229.78***	298.11***	176.13***	113.89***	78.70**	116.14***
	(34.67)	(65.55)	(22.62)	(15.95)	(37.32)	(14.13)
I_i^{Family}	7.81	-16.93	10.66	0.45	11.27	-8.07
	(17.53)	(26.07)	(17.95)	(11.27)	(30.49)	(8.86)
$Post \times I_i^{Family}$	-59.42**	-84.71**	-21.76	-15.86	-32.75	-3.74
	(31.28)	(50.18)	(27.12)	(14.88)	(35.78)	(13.34)
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,188	523	665	1,328	277	1,051
Adjusted R^2	0.37	0.37	0.49	0.44	0.42	0.51

Notes: The table presents the change in credit spreads during the crisis for family and non-family firms for different loan types based on the heterogeneous exposure to the shock. The sample is divided into firms that maintain relationships with financial institutions highly exposed ($\Delta\tilde{L}_{i,s}$ is in the top 25% of the distribution) or less exposed (bottom 25%) to the Lehman collapse. $\Delta\tilde{L}_{i,s}$ measures the availability of bank credit at the firm level and is constructed following Chodorow-Reich (2014). The dependent variable is the credit spread on loans. I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{i,t-1} + \theta Z_{b,t-1} + \eta_t + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. Furthermore, I include controls for bank characteristics (Z). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_t are year fixed effects and η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.23: Demand Effects

	(1)	(2)
Variables		
	High Lehman	**Low Lehman**
$Post$	133.74***	149.72***
	(27.01)	(17.57)
I_i^{Family}	26.55	-5.63
	(23.47)	(11.55)
$Post \times I_i^{Family}$	-59.32**	-15.41
	(26.20)	(20.25)
Firm Controls	Yes	Yes
Bank Controls	Yes	Yes
Bank Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Observations	813	880
Adjusted R^2	0.47	0.42

Notes: The table focuses on the subsample of firms borrowing the same type of loan from the same lender during the crisis and presents the change in credit spreads during the crisis for family and non-family firms accounting for the heterogeneous exposure to the shock. The sample is divided into firms that maintain relationships with financial institutions highly exposed ($LehmanExposure_i$ is in the top 25% of the distribution) or less exposed (bottom 25%) to the Lehman collapse. Lehman Exposure measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). The dependent variable is the credit spread on loans. I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{i,t-1} + \theta Z_{b,t-1} + \eta_b + \eta_s + \varepsilon_{it}$, where y_{it} is the credit spread for loans taken out by company i in quarter t. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. Furthermore, I include controls for bank characteristics (Z). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.24: Family Control and Strictness

	(1)	(2)
Variables		
	High Lehman	**Low Lehman**
$Post$	-0.019	-0.014
	(0.015)	(0.011)
I_i^{Family}	-0.004	0.011
	(0.012)	(0.008)
$Post \times I_i^{Family}$	0.020	-0.014
	(0.013)	(0.012)
Firm Controls	Yes	Yes
Bank Controls	Yes	Yes
Bank Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Observations	1,332	1,453
Adjusted R^2	0.08	0.05

Notes: The table explores the change in covenant strictness during the crisis for family and non-family firms. The sample is divided into firms that maintain relationships with financial institutions highly exposed ($LehmanExposure_i$ is in the top 25% of the distribution) or less exposed (bottom 25%) to the Lehman collapse, so that I compare family and non-family firms with similar exposure to the shock. Lehman Exposure measures the exposure to the liquidity shock at the firm level and is constructed following Ivashina and Scharfstein (2010). The dependent variable is the covenant strictness measure constructed following Murfin (2012) that reflects the probability that a firm violates any of the covenants over the next quarter. I report estimates of the following regression: $y_{it} = \alpha + \beta Post_{it} + \gamma I_i^{Family} + \delta Post_{it} \times I_i^{Family} + \zeta X_{i,t-1} + \theta Z_{b,t-1} + \eta_b + \eta_s + \varepsilon_{it}$. $Post_{it}$ is an indicator variable that takes the value 1 between the fourth quarter of 2008 and the end of 2010, and 0 otherwise. I_i^{Family} is a dummy variable that takes the value 1 for family firms, and 0 for non-family firms. X are firm controls. I control for size (logarithm of assets), cash flow, leverage, cash holdings, interest expense, and S&P rating. Furthermore, I include controls for bank characteristics (Z). Bank controls include assets, deposit ratio, capital ratio, profitability, and provision for loan losses. η_s are industry fixed effects. Finally, I add bank fixed effects η_b. In each column, I report estimated coefficients and their standard errors. Standard errors are robust to heteroskedasticity and are clustered by firm.
Significance Levels: *** p<0.01, ** p<0.05, * p<0.1.

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